THURSDAY, APRIL 5, 1877

THE GEOLOGICAL SURVEY OF OHIO

Report of the Geological Survey of Ohio. Vols. I. and II. Geology, and I. and II. Palæontology. (Columbus, Ohio: Nevins and Myers, State Printers, 1873-5.)

IN the reports of American Scientific Surveys we have become accustomed to find that the results are as new and interesting as the methods of working are original and ingenious. Few of the States are more richly endowed with the elements of prosperity and stability than Ohio, and yet she has but recently come into the field with her contribution to the knowledge of her own geological structure and natural history. It is gratifying to know that this contribution can well afford to be tried by the high standard attained by the reports already issued by many of the neighbouring States.

The survey just completed is technically the second but practically the first geological survey of Ohio, taking into consideration how long ago the former survey was disbanded, and how short was its term of life.

In 1836 the legislature appointed a committee consisting of Dr. Hildreth, Dr. Locke, Prof. Riddell, and Mr. Lapham, to report on the best method of obtaining a complete geological survey of the State, and to estimate the probable cost. In the summer of that year three of these gentlemen made reconnaissances, while the fourth analysed iron ores and limestones. A year after the appointment of the committee, the legislature, on its recommendation, created a geological corps, comprising a state geologist (Prof. W. W. Mather) and six assistants. During the summer of 1837, the State geologist and three assistants prosecuted geological explorations, two assistants being absorbed by zoology and topography. Next summer the survey was continued on a similar footing, but a financial panic having broken out, "the legislature of 1838-39 made no appropriation for the continuation of the geological survey, and it was at once suspended." In spite of the disadvantages under which this early survey laboured in being almost entirely without palæontological assistance, its two annual reports were much appreciated, and the short-sighted economy that led to its disbandment was soon regretted by the citizens of the State. Although several attempts were made, what with the defalcations of a State treasurer, the building of a costly state-house, and the great Civil War, Ohio was not financially in a position to re-establish a geological survey till the year 1869.

According to its constitution, this new survey was to be begun (and was begun) on June 1, 1869, and was to be finished in three years. It was required "to make a complete and thorough geological, agricultural, and mineralogical survey of each and every county in the State." To the chief geologist the act of legislature allowed three assistants, and a number (generally eight or nine) of temporary local assistants were employed and paid from a fund provided for "contingent expenses." One of the assistant geologists was to be a chemist. We can see in the organic law of the survey no provision for a palæontologist, and presume that the appointment of that officer was authorised by one of the subsequent Acts of Appropriation. At any rate, Prof. J. S. Newberry, having been appointed chief geologist, conjoined with himself two assistants, a palæontologist and a chemist, and it can hardly be disputed that this was the best possible disposition of his forces, however desirable a large increase in the number of assistants might have been.

Such then is the scale on which a State, having an area of 39,904 square miles, plans its geological survey. Considering the number of working days in a year, the number of the field geologists, and the area of the State, many will not hesitate to decide that the character of a reconnaissance was stamped on the survey by its framers from the beginning. But it should not be forgotten that there are circumstances which render the geological mapping of Ohio exceptionally simple. The Palæozoic rocks which form its framework are so undisturbed, that over areas of sometimes thousands of square miles, only one formation makes its appearance at the surface, and outcrops are therefore little more than contour lines. We are accustomed in this country to think of "dip" as something visible to the naked eye, and measurable with a pocket clinometer; and as producing, by its relation to the contour of the ground, endless variety in our geological boundary-lines. In Ohio it appears that the method of ascertaining the degree of dip is to set up half a dozen or so of trigonometrical stations, several miles apart, and carefully take the levels of the outcrop at the several points. So far as we have noted in reading the Reports, there is not a dip in the whole state that would make an appreciably stiff railway gradient. Then we are informed by Prof. Newberry that "faults in which the displacement amounts to more than one foot are very rare in the Ohio Coal-field," and that the greatest known has a down-throw of 3 feet. In Europe the complications produced by faults frequently add the excitement of a puzzle to the labours of the field-geologist and just as often leave an irritating element of uncertainty to embitter the satisfaction with which he is apt to regard his finished work. Then again there are no igneous rocks in Ohio and no metamorphic rocks in the ordinary sense of the term. Indeed it may almost be said that over large tracts there are no rocks at all. Thus in one county, "consisting of twelve towns," *i.e.* 432 square miles—exactly the area, let us say for comparison's sake, of a whole sheet of the 1-inch Ordnance Map of Scotland—the rock is deeply covered with drift and is never seen, having been reached by boring at one point only, at a depth of 110 feet.

On the other hand, the very simplicity of the geology of the State makes it a typical region by which other lands may measure their geological scale, and on this account it becomes necessary to survey it with minuteness and care. If Ohio renders this service to the neighbouring states, each of these has already given an equivalent. As it happened, when the late survey was begun, Ohio was almost surrounded by a belt of states which had got ahead of her in the work, and whose completed labours greatly simplified her task, at the same time that they presented discrepancies which could only be reconciled on her neutral ground.

Although it was originally intended that the survey should be finished in three years, its field work lasted for five, the average annual cost being $17,355.

We have now before us a portion of the final Reports of the Survey. These consist of two volumes of Geology, two of Palæontology, and two of Sheets of Vertical Sections (a sheet of Vertical Sections, by the way, is called a "Map" in the American language). Besides these there have already been published three volumes of Annual Reports of Progress, two of them containing Accounts of the Geology of Counties, in substantially the same form as that followed in the Final Reports. A third volume of Geology, to comprise by far the most important counties in the coal-field, is ready for publication and "awaits the action of the Legislature." "The matter for the third volume," says Prof. Newberry, "has been, to a considerable extent, prepared since the appropriations for the salaries of the geological corps were discontinued. Much of it is, therefore, a gratuitous contribution with which the corps should be credited when a comparison is made between the value of their services and the compensation they have received." Materials for a third volume of Palæontology have been accumulated, but the chief geologist does not speak confidently of his chances of getting a grant to defray the cost of its publication. It would be a thousand pities that the State which has been at the expense of collecting this information should not secure the credit and advantages involved in its official publication. Otherwise, the materials will have to be hunted through the transactions of American and foreign societies, and will be as good as half lost.

A volume on Economic Geology is far advanced, but six months' time and from $4,000 to $5,000 are estimated as necessary to complete it. A volume on the Zoology and Botany of the State is also ready for publication at a trifling expense.

Lastly, "a general geological map of the State can be prepared at a cost not greater than $1,500."

It will thus be seen that there still remain to be published some of the absolutely essential parts of the work of the survey. For scientific purposes the geological map stands first in point of necessity. It is not too much to say that it is chiefly by its general geological map that the survey will be known and judged abroad. It is indeed possible to construct a sort of geological map from the county and other sketch maps and information scattered through the volumes, and the writer has done so for his own satisfaction and as a means of mastering the Reports. But we can affirm with confidence that this is a labour that few will undertake, and which it would be much better that the officers of the Survey should perform once for all. The county maps, as will readily be understood from what has been said about the undisturbed condition of the rocks, are simplicity itself, being generally rectangles, crossed on an average by three boundary-lines inclosing four dabs of colour. But the general map will doubtless contain outcrops of coal and ironstone seams, the positions of oil and brine wells, fossil localities, and numerous other details, whose bearings can only be properly estimated when seen in the mass, or which it is the function of a geological survey to record, since for economic purposes the registration of all mining enterprises, whether failures or successes, is of permanent value. In her own interests we cannot doubt that the State will at once provide for the publication of the volume on Economic Geology, depending as she does to a large extent at present, and as she is certain to do still more in the not very distant future, on her mining industries.

If we may judge of the promised volumes on Palæontology, Zoology, and Botany from the results already before us, we are confident that their publication will place scientific workers in Europe as well as America under a debt of gratitude to Ohio, and we trust they will not be withheld.

It appears that when the first volume of the Final Report was ready, the Legislature ordered that the edition should consist of four times the number of copies estimated by the chief geologist as likely to meet the public demand. It is to be hoped that a similar spirit will induce them to complete the Survey's publications. A survey by four geologists, in three years, of a country one-third larger than Scotland, must soon have been felt to be impracticable, more especially if it was meant that the whole of the surveyor's labours, writing as well as field work, were to be compressed within the three years over which it was originally planned that their salaries were to be continued. A conscientious desire to finish their work having kept the officers of the survey in the field (doubtless with the approval of the legislature) for two years beyond the estimated time, the results of their zeal and skill ought not to be thrown away. There need be no hesitation in admitting Prof. Newberry's assertion, when he "claims" "that an honest and energetic use was made of the time and money expended on the Survey, and that its fruits will be worth much more than their cost to the people of Ohio."

We must refer the reader to the Reports themselves for the valuable information with which they are crowded. We can only notice briefly the leading scientific results, and some points of more than usual interest.

The rock-formations exposed in Ohio form an almost unbroken series, ranging from Lower Silurian to Carboniferous, inclusive. The principal feature in the geology of Ohio is undoubtedly the "Cincinnati axis," and to this the late Survey has justly devoted much attention. This great arch, passing through Cincinnati and the west end of Lake Erie, brings to light the oldest rocks of the State. It has hitherto been understood to be a minor flexure of the same date as the elevation of the Appalachian chain, to which it is, roughly speaking, parallel. But the investigations of the recent Survey have proved it to be much older. While the Appalachian chain does not appear to have been elevated until after the Carboniferous epoch, Prof. Orton has made the discovery that a large portion of the Cincinnati region was a land-surface, and suffered erosion towards the close of Lower Silurian times. The denudation of a synclinal arch and consequent exposure of deep-seated strata would not, of course, alone suffice to prove this; but the insular character of the Lower Silurian land of Cincinnati is rendered certain by the occurrence of pebbles derived from it in conglomerates at the base of the Upper Silurian deposits, and by the manner in which calcareous strata of that age, as as well as some Devonian limestones, thin out on approaching what must have been the shores of the island. It is not so clear whether the island was or was not entirely submerged in Devonian and Lower Carboniferous times. On the other hand, Prof. Newberry and his col-

leagues have satisfied themselves that during the deposition of the Upper Carboniferous or Productive Coal-measures, the Cincinnati land formed a barrier between the marshes of Ohio and Indiana; in other words, that the Alleghany and Illinois Coal-fields were never united, at least as far south as Alabama and Arkansas, where wide-spread tertiary deposits obscure all evidence bearing on the point.

"It is important," says Prof. Orton, "to mark the following fact distinctly, viz., that there is quite a broad tract at the summit of the fold in which the beds have but little dip. It is hard to speak of an axis without involving the idea of a line, but there is probably no part of this region of less than a score of miles that deserves, by way of excellence, the name of the Cincinnati axis. In other words, this fold has a broad and flat axis, rather than a linear one." The elevation has been so gentle and so gradual that direct visible evidence of unconformable succession is hardly to be expected.

No reader will fail to be struck by the important place accorded to chemical geology in the Reports. This portion of the work has been done mainly by Prof. Wormley, and adds greatly to the value of the survey as a whole. He has not confined his investigations to minerals of immediate economic importance, but has placed on record many analyses that must for a long time to come be drawn upon with advantage as the development of the resources of the State goes on. Especially as regards limestones and cement-stones and the amount of sulphur in the various coal-seams, very complete and useful information is given.

More than a score of counties "lie wholly within the limits of the productive coal-measures," and of nearly as many the geological surveyors pronounce without hesitation that "the soil will necessarily always be the source of their greatest material wealth." It sounds strange to hear already from such a rich agricultural district as Western Ohio the cry of exhaustion of the soil, but as all the surveyors without exception sound the note of warning against unskilful farming, it is evident that ere long science will have to be called in to assist nature if the productiveness of the State is to be maintained.

Although doubtless to be discussed more fully in the volume on economic geology, the coal and ironstone seams of the great coal-field, and the salt, oil, and gas industries receive much attention in the various county Reports. Prof. E. B. Andrews furnishes a chapter on coal which is full of interesting facts. Mr. M. C. Read gives a plan of a coal-mine in Trumbull County, which shows how very local was the formation of the seam. The coal thins out on every side, and presents the outline of a long winding swamp with branching creeks.

The importance of the ironstone beds in Ohio is well known. A black-band in Tuscarawas County locally attains, according to Prof. Newberry, a thickness of 12 feet.

The excitement caused by the discovery of the oil-wells of Pennsylvania and Ohio will yet be fresh in the memory of our readers. The conditions under which petroleum occurs are well illustrated in the Reports. There must be a mass of carbonaceous shales from whose organic contents the hydro-carbons are slowly distilled, and an overlying porous rock for the storage of the products—best of all a jointed sandstone with an impervious stratum for a roof—if dome-shaped so much the better. When these conditions are present the oil is ready for the fortunate landowner, and his luck is the greater if he happen to strike a joint where a quantity can collect. So well is this now understood that when a well shows symptoms of giving out, a torpedo is exploded in it to loosen up the rock and open out the way to neighbouring fissures. Carbonaceous shales, yielding oil, are met with at various horizons from the Huron (Devonian) upwards.

Carburetted hydrogen gas occurs under similar conditions and is now expressly bored for. The town of Fredonia, N.Y., has been lighted up with natural gas for more than forty years. In Knox County, Ohio, two wells were sunk to the Huron shale. "At a depth of about 600 feet, in each well, a fissure was struck from which gas issued in such volume as to throw out the boring tools and form a jet of water more than 100 feet in height. . . One of these wells constantly ejects, at intervals of one minute, the water that fills it. It thus forms an intermittent fountain 120 feet in height. The derrick set over this well has a height of 60 feet. In winter it becomes encased in ice, and forms a huge translucent chimney, through which, at regular intervals of one minute, a mingled current of gas and water rushes to twice its height. By cutting through this hollow cylinder at its base and igniting the gas in a paroxysm, it affords a magnificent spectacle, a fountain of mingled water and fire which brilliantly illuminates the icy chimney. No accurate measurement has been made of the gas escaping from these wells, but it is estimated to be sufficient to light a large city." Unfortunately there is no large city to light.

Geologists had a right to expect from Ohio an important contribution to their knowledge of the Glacial period, and Prof. Newberry and his colleagues have not disappointed them. The chief geologist sums up the results of the Survey in a masterly essay, and it is satisfactory to find that his views to a great extent corroborate the conclusions at which glacialists in Europe have arrived. Want of space compels us to allude to these in the briefest manner. The cold came on at a period when the land stood considerably higher than at present, as is proved by numerous river channels deeply buried beneath the drift. A wide-spread boulder clay or hard-pan, the product of a land ice-sheet radiating from the Canadian Mountains marks an early, and the greatest, development of the cold. A subsidence followed on the retreat of the ice-sheet, and a stratified clay was deposited over low-lying portions of the hard-pan. Then a forest covered a large portion of the glacial *débris*, and this furnishes remains of the mammoth, mastodon, and giant beaver. Another submergence covered the forest-bed with the loess of the Mississippi Valley, and icebergs strewed boulders from Canada over the State. Much of the older drift was reassorted and heaped up into kaims or eskars. Lastly, the sea gradually retired, occasionally pausing, and giving rise to terraces in the river valleys.

Intimately connected with the Glacial period were the hollowing out of the great lake-basins, and numerous important changes in the drainage-system of the continent. Taking Lake Erie as the simplest case, it is clear that its

basin was not excavated during the greatest extension of the ice-sheet, which, as shown by the striæ on the higher ground, passed directly *across* the valley. But in the bottom of the valley the striæ point *up* the lake, and this fact makes it probable that the excavation of the basin was the work of local ice, in other words, that it dates from a time when the valley-glaciers had ceased to coalesce. The islands near the upper end of the lake are wrought out of hard Corniferous sandstone and Water-lime exposed on the crown of the Cincinnati anticline. This hard barrier, Prof. Newberry believes, opposed an obstinate resistance to the passage of the glacier, and was consequently left in comparative relief.

The Ohio geologists without exception appear to be sub-aërialists, and indeed, the scenery of the State—such as it is—could hardly admit of any other explanation. It would not be easy to connect valleys of some hundreds of feet in depth with faults of less than a yard.

Of the palæontology of the Reports, we need only say that it is a remarkable proof of the enthusiasm, energy, and success of the late Prof. Meek and the naturalists who assisted him, several of them without any compensation. The publication of the Survey as a whole marks an epoch in culture as well as in material progress, in which all the well-wishers of the State must rejoice.

OUR BOOK SHELF

History of Nepāl. Translated from the Parbatiyā, by Munshi Shero Shunker Singh and Pandit Shrī Gunānand. With an Introductory Sketch of the Country and People of Nepāl, by the Editor, Daniel Wright, M.A., M.D. (London, Cambridge Warehouse; Cambridge, Deighton, Bell, and Co., 1877.)

THE Cambridge University Press have done well in publishing this work. Such translations are valuable not only to the historian but also to the ethnologist; perhaps more so to the latter than the former, as the very myths with which a people are apt to adorn their own history may become, in the hands of a cunning ethnologist, a clue to their racial connections. Dr. Wright's Introduction is based on personal inquiry and observation, is written intelligently and candidly, and adds much to the value of the volume. The coloured lithographic plates are interesting.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

The First Swallow at Menton

THE first swallow arrived here alone in the rain on Monday, March 19. It entered the best room of the *curé* by one of the windows which chanced to want a pane, and the good old man immediately removed a pane from the other window, by which the swallows have been in the habit of going in and out. I did not hear of the arrival of this summer resident until the 23rd, when I immediately paid it a visit. It is still solitary but not uncomfortable; it flits about the room from place to place, and from nest to nest, twittering very contentedly; and when a bright hour comes it flies out, where, sporting in the sun it soon makes a hearty meal. But it has arrived decidedly too soon, for it has found as yet mostly wet and rather cold days with snow-covered mountains for its immediate surrounding. Such, however, is the climate of this place, difficult to conceive by untravelled Englishmen, that I at this moment bask outside in the sun, soothed by the singing of birds, surrounded by flowers and butterflies, and the green trees with their golden fruits. I am in the midst of summer, and yet I have but to turn my head, and there, close at hand, are the mountains white with snow.

The coldest weather we have had this winter began with this month. The only time I have seen ice was on the morning of March 1. (On the preceding night, I see by a letter to NATURE, vol. xv. p. 399, that the thermometer at the Stonyhurst Observatory went down to 9°·1 F., the lowest temperature there recorded during the last sixteen years.) That morning, cheated by the serene stillness and the bright sunshine, I, before getting out of bed, resolved to make a journey to the sea-side—a distance of about three miles. A lunch was immediately packed up and the donkey of the *curé* borrowed for the occasion. As soon as I descended into the valley—Cabrolles, consisting of some dozen houses, all the dwellings of peasants, and hung on the mountain side like so many birds' cages or birds' nests on the back wall of a court, open only to the south, is 300 feet above the level of the sea, and enjoys a climate superior to that of the much-vaunted Menton. I am, however, the first *stranger* who has ventured to brave the isolation, the inconvenience, and want of accommodation.—Well, as I have said, on descending into the valley, a change of temperature suggested that it would be preferable to have my Italian cloak around me, instead of carrying it before me on the donkey. Proceeding a little farther, I saw with astonishment large quantities of ice in the torrent, and in turns of the road looking northward, icicles, thick as my arm—which, however, is one of the thinnest—hanging from the rocks. Still I went forward quite irrationally, carried along solely by the force of the impetus with which I started, for, as I approached Menton, I had to make way in the face of a biting cold wind. But I would certainly have shivered over my cold lunch among the rocks or ruins at Cap Martin, had not my progress received a check at Menton, in the for the moment irritating discovery that the key of the provision-bag had been, I may now say providentially, lost. I accepted the hospitality of a kind English clergyman, who gave me a nice warm lunch, after which I slowly wound my way back to my mountain retreat, where I dwell almost as completely removed from the winter visitants of these shores as is the now lonely swallow from its companions, the summer visitants, which have not unwisely made a halt somewhere by the way.

After this long digression I must return for a moment to the swallows of Cabrolles. They live in the rooms with the people, attaching their nests generally to the beam which supports the ceiling. On their arrival, whether it be by night or by day, they enter at once and take possession of their old habitations. Madame Valetta, an old woman of seventy-three, has two or three times given me a graphic account of how, when she was a young woman and had her husband by her side, they were both frightened almost to death one night by something which from time to time gave a flap-flap against the glass of the window. Madame, however, summoned courage to urge her husband to get up and open the window, which, though "all of a shake," he did, when whish! very like a spirit, a weary swallow glided past him and was the same instant peacefully reposing in its nest. DOUGLAS A. SPALDING

Cabrolles, près de Menton, France,
March 24

Coal Fields of Nova Scotia

IN his address to the Iron and Steel Institute (NATURE, vol. xv., p. 462), Dr. Siemens stated that the area of the Coal Fields of Nova Scotia was 18,000 square miles, and the production in 1874 1,052,000 tons. If Dr. Siemens will refer to Dr. Dawson's "Acadian Geology," the Reports of the "Canadian Government Geologists," and Brown's "Coal Fields and Coal Trade of Cape Breton," he will find that he has greatly overstated the area of the Nova Scotia Coal Fields. From these sources, which I believe are perfectly reliable, I make out that the whole area of the Nova Scotia Coal Fields does not amount to 1,000 square miles, distributed over the following counties:—

	Square Miles.
Cumberland	250
Picton	34
Cape Breton	194
Victoria	6
Inverness	40
Richmond	10
	534

To this amount, however, must be added the portions of the Cape Breton and Inverness Coal Fields lying under the sea, which, supposing the seams can be worked a distance of five miles beyond high-water mark, will make the total area of the Nova Scotia Coal Fields 859 instead of 18,000 square miles.

The production in 1874 has also been greatly overstated in Dr. Siemens' Table, as I find by reference to the Government Inspector's Report for the year 1876, that the production in 1874 was only 749,127 tons. R. B.

[Dr. Siemens informs us that the difference referred to by our correspondent chiefly arises from the fact that in American reports Nova Scotia is made to include the maritime province of New Brunswick as well as Cape Breton Island, both of which contain large areas of coal-fields, although those fields are as yet very imperfectly developed. The figures given in the address were taken from Macfarlane's very elaborate work on the "Coal-Regions of America." With reference to the coal production, this should be for the year 1873, and is also given on the authority of Macfarlane, who quotes from the Report of the Department of Mines.—ED.]

Greenwich as a Meteorological Observatory

A CAREFUL examination of the interesting communication by Mr. Alexander Buchan to the Scottish Meteorological Society, on "The Temperature of the British Islands," based on observations for the thirteen years ending 1869, fails to support his conclusion (NATURE, vol. xv., p. 450) that the proximity of London does not appreciably influence the temperature as recorded at the Royal Observatory, and that the temperature of Greenwich during recent years has not been in excess of that of surrounding districts. The evidence is quite the reverse. Extracting the figures, given by Mr. Buchan in the paper referred to, for all the stations within a radius of sixty miles of the metropolis, sixteen in number besides Greenwich, it appears that their mean is 50°·1, that of Greenwich being 50°·6. Allowing for elevation, the values are respectively 50°·68 and 51°·13. Omitting, however, several stations such as Camden Town, which, forming part of London, is clearly inadmissible for the comparison, and Maidstone and Canterbury, where observations were made on two years only of the thirteen, the temperature of the ten remaining stations is 50°·59. Thus, according to data furnished by Mr. Buchan himself, Greenwich is warmer than the south-east of England generally by more than half a degree (0°·54). It may be added that, from the same data, the temperature of the district under consideration north of the Thames is 50°·5, and south of the river 50°·8. H. S. EATON

Centralisation of Spectroscopy

IN his letter (NATURE, vol. xv. p. 449) Prof. Smyth makes a statement respecting the new "half-prism" spectroscope which I cannot help thinking must be founded on a misapprehension of the principle involved. This will, I trust, be made clear when my paper is published in the forthcoming number of the *Proceedings* of the Royal Society; but meanwhile, as Prof. Smyth appears disinclined to wait for a full explanation of the instrument, I shall be most happy to answer his objections when he informs me what particular "laws of Sir Isaac Newton and nature" are in opposition to the principle of this spectroscope.

Against Prof. Smyth's confident assertion that all definition is lost in this instrument, which he has never seen and of which he can only know by hearsay, I have only to set the statement that a small experimental spectroscope on the new plan, with two "half-prisms," is, as a matter of fact, decidedly superior in definition, as well as in brightness of spectrum, to the large Greenwich spectroscope, with ten large compound prisms, of which the excellence is sufficiently attested by the accordance of the results obtained for the sun's rotation by its means. This statement is based on a careful comparison of the sodium lines, and also of the *b* group in the solar spectrum, as seen with the two instruments, b_3 and b_4 with the finer lines in their neighbourhood being shown with remarkable distinctness in the new form of spectroscope, small though it is. In this assertion, I think, I shall be fully borne out by several astronomers to whom I have shown the action of the new spectroscope.

Though I am not in any way concerned with Prof. Smyth's argument in the earlier part of his letter, I may mention for his information that "during the last twenty years" only *two* spectroscopes have been made for Greenwich Observatory (one of these having only a single prism of small dispersion), and that our second or powerful spectroscope was only made *three* years ago; whilst the Edinburgh observatory has, for the past *four* years, possessed *three* spectroscopes which are almost precisely identical with those used with such effect by Dr. Huggins.

W. H. M. CHRISTIE

Royal Observatory, Greenwich, March 27

Morphology of "Selaginella"

BEFORE instituting a comparison it is generally prudent to ascertain that the things to be compared are comparable. I am afraid Mr. Comber, who has done me the honour of making some remarks on what I have said in the pages of NATURE on the primordial type of flowers, has neglected this precaution. If I understand him rightly, he suggests that the "spike" of *Selaginella* is the homologue of the spike of *Carex pulicaris*. He compares, then, the scales bearing macrosporangia of the former with the lower glumes bearing each an ovary of the latter.

Now in the first place, if he had studied the matter a little more (if he will allow me to say so), he would have seen that the ovule, and not the ovary, is the equivalent of the macrosporangium, and that the embryo sac, and not the ovule, is the equivalent of a macrospore. This leaves the ovary unaccounted for, and the homology hopelessly breaks down on that point.

But this is not all. Mr. Comber has omitted all notice of the singular structure, the perigynium, and also of the equally singular structure, the "seta," which it contains along with the ovary, and which happens to be particularly well represented in *Carex pulicaris*. If he will take the trouble to look at a short paper which I have published in the *Journal* of the Linnean Society (Botany) vol. xiv., pp. 154-156, pl. xii., he will find that I have carefully discussed the morphology of the female flower of this very plant. I think I have succeeded in showing that far from being a simple racemose inflorescence it is a compound raceme or panicle reduced in a very peculiar manner. I am afraid, therefore, that Mr. Comber has been led away by resemblances of a very superficial character, and that the fact *Selaginella* has a "spike" and that *Carex* has a "spike," is a point of contact between the two about as significant as the existence of a river in both Macedon and Monmouth.

In fact, far from being plants of a primitive type, the *Cyperaceæ* are generally regarded as reduced representatives of plants of much more fully developed character, the exact nature and relationship of which we have no materials for at present estimating. W. T. THISELTON DYER

Tungstate of Soda

WITH regard to your note (NATURE, vol. xv. p. 460) upon muslin rendered uninflammable by tungstate of soda, will you allow me to say that when properly prepared the muslin is fairly uninflammable. I say uninflammable—not fireproof. There can be no doubt from experiments made in Prof. Gladstone's laboratory that muslin prepared with a sufficient quantity of the salt will not catch fire by ordinary means, but no one could reasonably expect it to stand an *auto da fe* such as that to which I saw Dr. Wright subject his dummy, and fortunately not his assistant, last Saturday fortnight. MATTHEW W. WILLIAMS

Chemical Laboratory, Royal Institution

Traquair's Monograph on British Carboniferous Ganoids

WILL you kindly permit me through the medium of your journal, to correct and apologise for a very awkward blunder which occurs in the first part of my monograph on British carboniferous ganoids, recently published by the Palæontographical Society? In the introduction I have advocated the retention of the *Dipnoi* as a distinct order of fishes, but at page 41, in a manner unaccountable to myself, for I certainly did not mean it, I have included them as a sub-order of the Ganoidei. That this "slip of the pen" was not detected in the revision of the proofs must have been due to an amount of carelessness of which I am justly ashamed. R. H. TRAQUAIR

Edinburgh, April 2

ALEXANDER BRAUN

WE regret to announce the death of the well-known German botanist, Prof. Alexander Braun, which took place at Berlin, on March 29. He was born in Ratisbon, May 10, 1805, and after the completion of his university studies entered upon the duties of Professor of Botany in the University of Freiburg, in Baden. Here he published his first important book, "Vergleichende Untersuchung über die Ordnung der Schuppen an den Tannenzapfen," in which he formulated the theory with regard to the position of the leaves on plants now essentially recognised by botanists. In 1850 he accepted a call to the University of Giessen, and issued shortly after his most notable work, "Betrachtungen über die Erscheinung der Verjüngung in der Natur, insbesondere in der Lebens- und Bildungsgeschichte der Pflanze." The extensive series of observations, and the numerous valuable theoretical deductions recorded in this suggestive work, formed one of the most noteworthy contributions to vegetable morphology, and placed the author at once among the leading botanists of the day. In 1852 he removed to Berlin, where he had been appointed Professor of Botany and Director of the Botanical Gardens, positions which he occupied up to the time of his death. The unwearied activity of Braun during this period is evidenced by the large number and variety of the contributions made by him to botanical literature. Of these his investigations on cryptogamia assume the foremost rank, embracing papers on the families *Marsilia*, *Pilularia*, and *Selaginella*, African varieties of *Chara*, Movements of the Juices in the Cells of *Chara*, Vegetable Individuals in their relations to Species, Some New Diseases of Plants caused by Fungi, New Varieties of Single-celled Algæ, &c.

Among his more prominent publications on phanerogamia should be mentioned the papers on parthenogenesis, polyembryony, and budding of *cœlebogyne*, and the oblique direction of woody fibre in its relations to twisted tree stems. His efforts in all investigations were chiefly directed to perfecting our knowledge of vegetable morphology, and by comparative studies in this region, to the establishment of well-defined laws with regard to the growth of plants, and the relationship between different varieties. Braun's theories on the latter subject led to the formation of a system, which, although not accepted in all points, is yet regarded by many botanists as the most perfect approach to a natural classification of plants which we at present possess. A contemporary botanist describes the leading feature of his character as consisting in an "earnest striving to bring all the widely diverse families of the vegetable kingdom, fossil as well as existing, within his grasp, and by means of thorough, comparative study to advance toward the true natural classification."

The merits of Prof. Braun were recognised by the bestowal of numerous German orders, and from the King of Prussia he received the title of "Geh.-Regierungs-Rath." He was a prominent member of the Berlin Academy of Sciences and the Botanical Society, occupying the presidency of the latter for a number of years. His papers appeared chiefly in the *Transactions* of these two societies; the classification of plants being given, however, in Ascherson's "Flora of the Province of Brandenburg," in 1864.

THE LOAN COLLECTION OF SCIENTIFIC APPARATUS

THE last of the "present series" of free lectures in connection with the Loan Collection of Scientific Apparatus was given on Saturday, in the lecture theatre of the South Kensington Museum. Major Festing, R.E., took the chair, and the theatre was, as usual, crowded. The lecture was given by Mr. W. Stephen Mitchell, M.A., on "The *Challenger* Soundings and the Lost Island of Atlantis." An abstract of this will shortly appear. At the end of the lecture Mr. Mitchell said he thought that as this was the last—at any rate of this series—it would be in accordance with the wish of the audience that a few words should be said by way of *résumé*, to mark the occasion. He regretted that his place was not occupied by some one eminent in science. When the Loan Collection of Scientific Apparatus was opened there were planned in connection with it conferences, demonstrations, lectures to science teachers, and the free evening lectures. The conferences lasted as planned during May and June, the lectures to science teachers were carried out as proposed, and the demonstrations were given till December 31. At that date, in consequence of packing the cases for returning the collections lent from abroad, which were lent for a definite period only, it was necessary to close the galleries to the public. The free lectures, however, had been continued, and the apparatus from the galleries had been brought into that theatre, as it had been found necessary, to illustrate the lectures. The lectures had thus kept up the continuity of the collection. He believed he was right in saying that from the outset the promoters of the Loan Collection had looked forward to the establishment of a permanent physical science museum somewhat in imitation of the Conservatoire des Arts et Métiers of Paris. Such a museum was recommended by the Royal Commission on Scientific Education, under the presidency of the Duke of Devonshire, and composed of some of the most distinguished men of science in this country. For a building to contain such a museum the commissioners of the Exhibition of 1851, under the presidency of the Prince of Wales, have voted 100,000*l.*, and offered it to the Government. A petition in favour of the establishment of such a museum had, since the opening of the collection, been signed by officers and fellows of learned societies, and presented to the Duke of Richmond and Gordon. At this last lecture of the series they would naturally ask what was likely to be done for the future. As he was in no way officially connected with the museum he was not in a position to give any certain information; but this much he could tell them, a number of instruments that would otherwise have been returned had been acquired by purchase, a number had been presented, a number were left on loan for an indefinite period, and many were left under certain conditions. The galleries at the present time contained a collection of fair size to commence a permanent collection. Here, as in considering the lost island of Atlantis, they must be careful to discriminate between facts and inferences to be drawn from facts. No announcement had been made by the Government as to its intentions. The present condition of the Collection, as he had stated it, was a fact, and they would draw for themselves inferences as to what this might mean. He had seen a statement that the permanent museum might be open in May, but he could not say how far this represented official intentions. The crowded audiences at the lectures in that theatre was, he said, a proof that they wished the Collection and the lectures in connection with it to continue.

Mr. F. S. Mosely moved, and Mr. J. Heywood, F.R.S., seconded the following resolution:—"We who form the audience at this, the last of the present series of lectures in connection with the Loan Collection of Scientific Apparatus, desire to thank the Board of the Science and Art Department for having arranged this series of lectures. We would wish to take this opportunity to express the hope that the Loan Collection of Scientific Apparatus may lead to a permanent collection of a similar nature. We beg the chairman to convey the terms of this resolution to the head of the department."

The motion was put to the meeting and carried unanimously with loud applause.

Major Festing said that, as representing the department, he was sorry he could give no more information than the lecturer had. The Government had not yet announced its intention as to what it would do in the matter. It had lately had many other matters on hand. With regard to the lectures, it was felt that it was hardly fair to continue to ask men of science to give their services gratuitously, and until some arrangement for fees could be made, he thought the lectures would probably remain in abeyance. It would give him pleasure to forward the resolution so unanimously carried to the head of his department as requested.

THE DEVELOPMENT OF BATRACHIANS WITHOUT METAMORPHOSIS

METAMORPHOSIS, or the transition of the animal through an intermediate stage between the ovum and the adult, has hitherto been considered by modern naturalists a special characteristic of the Batrachians amongst the Vertebrates, and as one of the main features which distinguish them from the true Reptiles, with which they were formerly united. It is, therefore, surprising to learn, as we do from a recent communication of Dr. Peters to the Royal Academy of Sciences of Berlin, that there are cases in which no such metamorphosis takes place, and the young frog is developed directly from the egg without showing any signs of what is usually called the "tadpole" stage.

Dr. Peters's noteworthy discovery is based upon observations made by Dr. Bello, Herr Krug, and Dr. J. Gundlach, in Porto Rico, on the development of a West Indian tree-frog—*Hylodes martinicensis*, which seems to be not uncommon in Porto Rico, and is there generally known by the vernacular term *Coqui*.

Five years ago Dr. Bello stated[1] that a tree-frog in Porto Rico called *Coqui* was remarkable from the fact that the young came out of the eggs in a perfect condition, and fit for life in the air. "In 1870," he says, "I observed in a garden an example of this species upon a liliaceous plant, on which about thirty eggs were clustered together in a cotton-wool-like mass; the mother kept close to them as if she intended to incubate. A few days afterwards I found the little frogs from two to three lines long just born, with all their four feet perfectly developed, springing about, and enjoying life in the air. In a few days they attained their full size. This garden is surrounded by walls six feet high, and there is no water in it. The so-called lily (which appears to be an introduced species of *Crinum*) always contains a little water in the receptacles, but is not a water-plant."

The translator of these observations rightly remarks that the exclusion of the animal out of the egg was not actually witnessed in this case, and that it was possible even in the short time which elapsed between when the eggs were seen and the young frogs appeared, some metamorphosis might have taken place, especially as the subsequent development seems to have been uncommonly quick.

These short observations of Dr. Bello appear to have attracted the less attention inasmuch as the development of tree-frogs from eggs placed in dry situations in frothy masses had been already observed and described in tropical countries. In 1867 Herr Hensel published some interesting observations on *Cystignathus mystacinus*, in the forests of Rio Grande do Sul,[2] and last year Dr. Peters laid before the Academy of Berlin the extraordinary discoveries of Buchholz concerning the egg-masses of *Chiromantis guineensis* laid upon trees in Guinea. Besides this the development of *Alytes obstetricans* between the hind-legs of the male in the ordinary way, and, through Herr Weinland's brilliant investigations, the metamorphosis of the young in the dorsal sacs of the females of *Opisthodelphys* and *Nototrema*, were facts so generally known that it seemed highly improbable that any Batrachian should be developed without metamorphosis.

Under these circumstances it is of the greatest interest to be able to state that Dr. Bello's information has been fully corroborated by recent observations of Dr. Gundlach and by preparations which he has transmitted to Berlin.

"On May 24, 1876," Dr. Gundlach says, "I heard a singular call like that of a young bird, and went to see what it was. Between two large orange-blossoms I perceived a leaf-frog, and on taking hold of it, found I had captured three males and a female of the *Coqui*. On putting them into a damp glass, one of the males quickly placed himself on the female and grasped around her. Not long afterwards she had laid from fifteen to twenty eggs, which, however, mostly soon disappeared—perhaps eaten.

"There were subsequently laid five eggs, round, with a transparent covering, which I removed and placed on some wet slime. The inner yelk, of a whitish or pale straw-colour, contracts a little, and then the tail is seen forming.

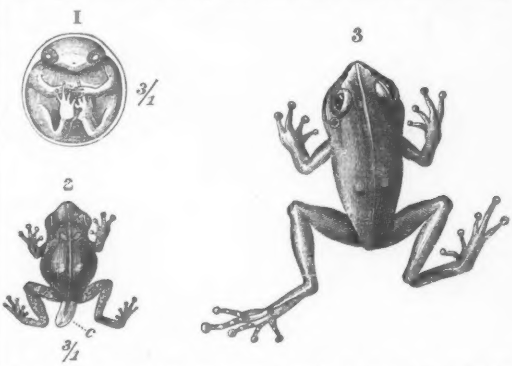

FIG. 1.—Egg of *Hylodes martinicensis*, twelve days old, lower surface FIG. 2.—Young of *Hylodes* as it leaves the egg; *c*, tail. FIG. 3.—Adult male *Hylodes*, natural size.

In eight days this was quite clearly visible, as well as the eyes, and the red pulsating blood-vessels. Later on traces of the legs became manifest. I was now absent for some days, and when I returned, on June 6, found the eggs still, but on the next morning, the young were out, and had no trace left of the tail.

"Afterwards I found between two leaves of a large Amaryllid, just like Dr. Bello, a batch of more than twenty eggs, upon which the mother was sitting. I cut off the leaf, along with the eggs—upon which the mother jumped off—and placed them in a glass with some damp earth at the bottom. About the fourteenth day, having returned from an excursion, I found, at 9 A.M., all the eggs hatched, and I remarked on the young ones a little white tail (see Fig. 2, *c*), which by the afternoon had altogether disappeared."

Dr. Gundlach's collection, as Dr. Peters tells us, contains four eggs of this frog, with embryos. They consist of a transparent vesicle of from 4·5 to 5·5 mill. in diameter, which is partly occupied by an opaque flaky white mass. The vesicle is filled with a transparent fluid, which allows one to see every part of the swimming embryo quite clearly. The embryo, as in the case of mammals, is curved together on the lower surface, so that the head approaches the lower extremities, which, as well as the anterior extremities, are drawn together under the belly and lie close to the body.

[1] "Zoologische Notizen aus Portorico," in "Der zool. Garten," 1871, p. 351.

[2] Sitz. d. Ges. Nat. Freunde zu Berlin 1867, p. 10, and Arch. . Nat. xxiii. pt. 1, p. 1[illegible].

The tail is likewise curved up underneath, and lies with its broad surface towards the body, turning either towards the right or the left, and thickening part of the hinder extremities. In three examples the extremities are fully developed, and even show the characteristic discs on the tops of the toes. In the fourth example all four extremities present short stumps, and as yet show no traces of toes, whereas, as is well known, in the *Batrachia anura* generally the hinder extremities and the ends of the feet first appear. Neither of branchiæ nor of branchial slits is there any trace. On the other hand, in the last-mentioned example, the tail is remarkably larger, and has its broad surface closely adherent to the inner wall of the vesicle, and very full of vessels, so that there can be no doubt of its function as a breathing organ. As development progresses, the yelk-bag on the belly and the tail become gradually smaller, so that at last, when the little animal, being about 5 mill. long, bursts through the envelope, the tail is only 1·8 mill. in length, and after a few hours only 0·3 mill. long, and in the course of the same day becomes entirely absorbed. Examples of the same batch of ova, which were placed in spirit eight days after their birth, have a length of from 7·0 to 7·5 mill., whence we may conclude that their growth is not quicker than in other species of Batrachians.

The development of this frog, Dr. Peters observes (and probably of all the nearly allied species), without metamorphosis, without branchiæ, with contemporaneous evolution of the anterior and posterior extremities, as in the case of the higher vertebrates, and within a vesicle, like the amnion of these latter, if not strictly equivalent to it, is truly remarkable. But this kind of development is not quite unparalleled in the Batrachians, for it has long been known that the young of *Pipa americana* come forth from the eggs laid in the cells on their mother's back tailless and perfectly developed. In them, likewise, no one has yet detected branchiæ, and we also know from the observations of Camper,[1] that the embryos at an earlier period are provided with a tail-like appendage, which in this case also, may be perhaps regarded as an organ of breathing, possibly corresponding to the yelk-placenta of the hag-fish. As regards this point, also, Laurenti says of the *Pipa :* "Pulli ex loculamentis dorsi prodeuntes, metamorphosi nulla ?" (Syn. Rept., p. 25.)

It would be of the highest interest, Dr. Peters adds, to follow exactly this remarkable development on the spot. The development of the embryo of these Batrachians in a way very like that of the scaled Reptilia makes one suspect that an examination of the temporary embryonic structures of *Hylodes* and *Pipa* would result in showing remarkable differences from those of other Batrachians. The general conclusions which might be drawn from this discovery are so obvious, says Dr. Peters, in conclusion, that it would be superfluous to put them forward.

A subsequent communication of Dr. Peters to the Academy informs us that it had escaped his notice that M. Bavay, of Guadaloupe, had already published some observations on the development of *Hylodes martinicensis*.[2] According to his observations, on each side of the heart there is a branchia consisting of one simple gill-arch, which on the seventh day is no longer discernible. On the ninth day there is no longer a trace of a tail, and on the tenth day the little animal emerges from the egg. M. Bavay also observed the contemporaneous development of the four extremities, and hints at the function of the tail as an organ of breathing.

The observations of Dr. Gundlach, therefore, says Dr. Peters, differ in some respects from those of M. Bavay. It would be specially desirable, however, to ascertain whether the arched vessel on each side of the heart is really to be regarded as a gill-arch, or only as the incipient bend of the aorta.

[1] Comm. Soc. Reg. Götting. Cl. phys. ix p. 135 (1788).
[2] Ann. Sc. Nat. ser. 5, xvii., art. No. 16 (1873.)

TYPICAL LAWS OF HEREDITY[1]

WE are far too apt to regard common events as matters of course, and to accept many things as obvious truths which are not obvious truths at all, but present problems of much interest. The problem to which I am about to direct attention is one of these.

Why is it when we compare two groups of persons selected at random from the same race, but belonging to different generations of it, we find them to be closely alike? Such statistical differences as there may be, are always to be ascribed to differences in the general conditions of their lives; with these I am not concerned at present, but so far as regards the processes of heredity alone, the resemblance of consecutive generations is a fact common to all forms of life.

In each generation there will be tall and short individuals, heavy and light, strong and weak, dark and pale, yet the proportions of the innumerable grades in which these several characteristics occur tends to be constant. The records of geological history afford striking evidences of this. Fossil remains of plants and animals may be dug out of strata at such different levels that thousands of generations must have intervened between the periods in which they lived, yet in large samples of such fossils we seek in vain for peculiarities which will distinguish one generation taken as a whole from another, the different sizes, marks and variations of every kind, occurring with equal frequency in both. The processes of heredity are found to be so wonderfully balanced and their equilibrium to be so stable, that they concur in maintaining a perfect statistical resemblance so long as the external conditions remain unaltered.

If there be any who are inclined to say there is no wonder in the matter, because each individual tends to leave his like behind him, and therefore each generation must resemble the one preceding, I can assure them that they utterly misunderstand the case. Individuals do *not* equally tend to leave their like behind them, as will be seen best from an extreme illustration.

Let us then consider the family history of widely different groups; say of 100 men, the most gigantic of their race and time, and the same number of medium men. Giants marry much more rarely than medium men, and when they do marry they have but few children. It is a matter of history that the more remarkable giants have left no issue at all. Consequently the offspring of the 100 giants would be much fewer in number than those of the medium men. Again these few would, on the average, be of lower stature than their fathers for two reasons. First, their breed is almost sure to be diluted by marriage. Secondly, the progeny of all exceptional individuals tends to "revert" towards mediocrity. Consequently the children of the giant group would not only be very few but they would also be comparatively short. Even of these the taller ones would be the least likely to live. It is by no means the tallest men who best survive hardships, their circulation is apt to be languid and their constitution consumptive.

It is obvious from this that the 100 giants will not leave behind them their quota in the next generation. The 100 medium men, on the other hand, being more fertile, breeding more truly to their like, being better fitted to survive hardships, &c., will leave more than their proportionate share of progeny. This being so, it might be expected that there would be fewer giants and more medium-sized men in the second generation than in the first. Yet, as a matter of fact, the giants and medium-sized men will, in the second generation, be found in the same proportions as before. The question, then, is this:—How is it that although each individual does *not* as a rule leave his like behind him, yet successive generations resemble each other with great exactitude in all their general features?

[1] Lecture delivered at the Royal Institution, Friday evening, February 9, by Francis Galton, F R.S.

It has, I believe, become more generally known than formerly, that although the characteristics of height, weight, strength, and fleetness are different things, and though different species of plants and animals exhibit every kind of diversity, yet the differences in height, weight, and every other characteristic, are universally distributed in fair conformity with a single law.

The phenomena with which it deals are like those perspectives spoken of by Shakespeare which, when viewed awry, show nothing but confusion.

Our ordinary way of looking at individual differences is awry; thus we naturally but wrongly judge of differences in stature by differences in heights, measured from the ground, whereas on changing our point of view to that whence the law of deviation regards them, by taking the average height of the race, and not the ground, as the point of reference, all confusion disappears, and uniformity prevails.

It was to Quetelet that we were first indebted for a knowledge of the fact that the amount and frequency of deviation from the average among members of the same race, in respect to each and every characteristic, tends to conform to the mathematical law of deviation.

The diagram contains extracts from some of the tables,

Scale of Heights.	American soldiers, 25,878 observations.		France (Hargenvilliers).		Belgium, Quetelet. 20 years' observations.	
Metres.	Observed	Calculated.	Observed	Calculated.	Observed	Calculated
1·90	1	3				
1·90	7	5				
87	14	13		1	1	1
84	25	28		3	2	3
81	45	52	25	7	7	7
79	99	84		16	14	14
76	112	117	32	32	34	28
73	138	142	55	55	48	53
70	148	150	88	87	102	107
68	137	137	114	118	138	136
65	93	109	144	140	129	150
62	109	75	140	145	162	150
60	49	45	116	132	106	136
57	14	24		105	110	107
54	8	11		73		53
51	1	4		44		28
48		1		24		14
45			286	11	147	7
42				4		3
39				2		1
36				1		
	1000	1000	1000	1000	1000	1000

Degrees of Dynanometer.	Lifting power of Belgian Men.	
	Observed.	Calculated.
200	1	1
190	9	10
180		
170	23	23
160		
150	32	32
140		
130	22	23
120		
110	12	10
100		
90	1	1
	100	100

by which he corroborates his assertion. Three of the series in them refer to the heights of Americans, French, and Belgians respectively, and the fourth to strength, to that of Belgians. In each series there are two parallel columns, one entitled "observed," and the other "calculated," and the close conformity between each of the pairs is very striking.

These Tables serve another purpose; they enable those who have not had experience of such statistics to appreciate the beautiful balance of the processes of heredity in ensuring the repetition of such finely graduated proportions as those they record.

The outline of my problem of this evening is, that since the characteristics of all plants and animals tend to conform to the law of deviation, let us suppose a typical case, in which the conformity shall be exact, and which shall admit of discussion as a mathematical problem, and find what the laws of heredity must then be to enable *successive* generations to maintain statistical identity.

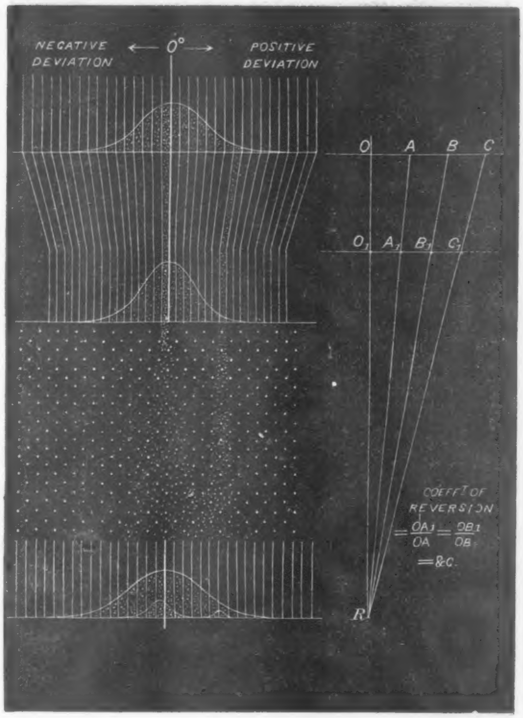

Fig. 1.

I shall have to speak so much about the law of deviation, that it is absolutely necessary to tax your attention for a few minutes to explain the principle on which it is based, what it is that it professes to show, and what the two numbers are which enable long series to be calculated like those in the tables just referred to. The simplest way of explaining the law is to begin by showing it in action. For this purpose I will use an apparatus that I employed three years ago in this very theatre, to illustrate other points connected with the law of deviation. An extension of its performance will prove of great service to us tonight, but I will begin by working the instrument as I did on the previous occasion. The portion of it that then existed and to which I desire now to confine your attention, is shown in the lower part of Fig. 1, where I wish you to notice the stream issuing from either of the divisions just above the dots, its dispersion among

them, and the little heap that it forms on the bottom line. This part of the apparatus is like a harrow with its spikes facing us; below these are vertical compartments; the whole is faced with a glass plate. I will pour pellets from any point above the spikes, they will fall against the spikes, tumble about among them, and after pursuing devious paths, each will finally sink to rest in the compartment that lies beneath the place whence it emerges from its troubles.

The courses of the pellets are extremely irregular, it is rarely that any two pursue the same path from beginning to end, yet notwithstanding this you will observe the regularity of the outline of the heap formed by the accumulation of pellets.

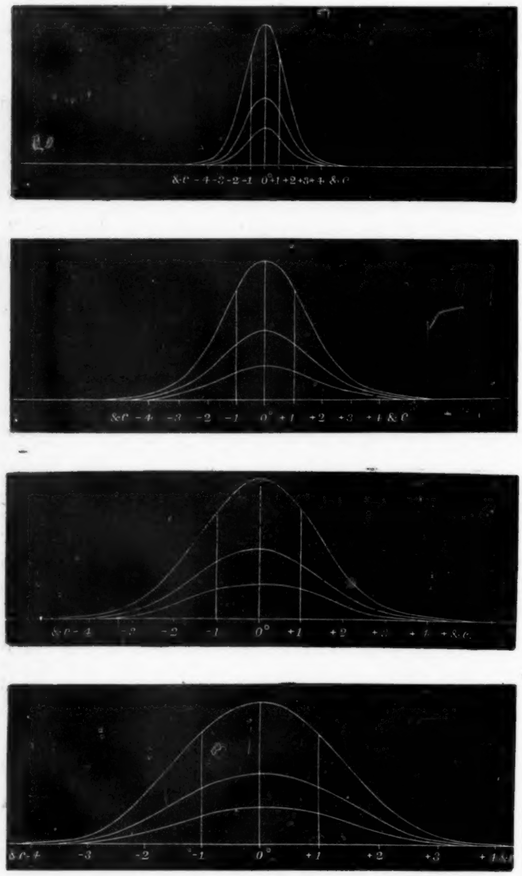

FIG. 2.

This outline is the geometrical representation of the curve of deviation. If the rows of spikes had been few, the deviation would have been slight, almost all the pellets would have lodged in a single compartment and would then have resembled a column; if they had been very numerous, they would have been scattered so widely that the part of the curve for a long distance to the right and left of the point whence they were dropped would have been of uniform width, like an horizontal bar. With intermediate numbers of rows of teeth, the curved contour of the heap would assume different shapes, all having a strong family resemblance. I have cut some of these out of cardboard; they are represented in the diagrams (Figs. 2 and 3). Theoretically speaking, every possible curve of deviation may be formed by an apparatus of this sort, by varying the length of the harrow and the number of pellets poured in. Or if I draw a curve on an elastic sheet of india-rubber, by stretching it laterally I produce the effects of increased dispersion; by stretching it vertically I produce that of increased numbers. The latter variation is shown by the successive curves in each of the diagrams, but it does not concern us to-night, as we are dealing with proportions, which are not affected by the size of the sample. To specify the variety of curve so far as dispersion is concerned, we must measure the amount of lateral stretch of the india-rubber sheet. The curve has no definite ends, so we have to select and define two points in its base, between which the stretch may be measured. One of these points is always taken directly below the place where the pellets were poured in. This is the point of no deviation, and represents the mean position of all the pellets, or the average of a race. It is marked as 0°. The other point is conveniently taken at the foot of the vertical line that divides either half of the symmetrical figure into two equal areas. I take a half curve in cardboard that I have again divided along this line, the weight of the two portions is equal. This distance is the value of 1° of deviation, appropriate to each curve.

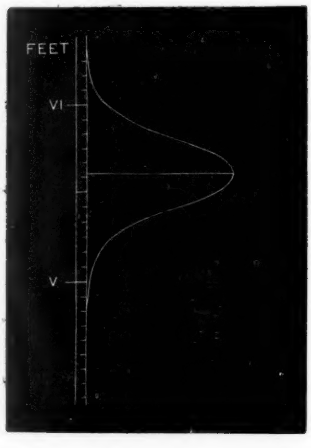

FIG. 3.

We extend the scale on either side of 0° to as many degrees as we like, and we reckon deviation as positive, or to be added to the average, on one side of the centre say to the right, and negative on the other, as shown in the diagrams. Owing to the construction, one quarter or 25 per cent. of the pellets will lie between 0° and 1°, and the law shows that 16 per cent. will lie between + 1° and + 2°, 6 per cent. between + 2° and + 3°, and so on. It is unnecessary to go more minutely into the figures, for it will be easily understood that a formula is capable of giving results to any minuteness and to any fraction of a degree.

Let us, for example, deal with the case of the American soldiers. I find, on referring to Gould's Book, that 1° of deviation was in their case 1·676 inches. The curve I hold in my hand has been drawn to that scale. I also find that their average height was 67·24 inches. I have here a standard marked with feet and inches. I apply the curve to the standard, and immediately we have a geometrical representation of the statistics of height of all those soldiers. The lengths of the ordinates show the proportion of men at and about their heights, and the area between any pairs of ordinates give the proportionate number of men between those limits.

It is indeed a strange fact that any one of us sitting quietly at his table could, on being told the two numbers just mentioned, draw out a curve on ruled paper, from which thousands of vertical lines might be chalked side by side on a wall, at the distance apart that is taken up by each man in a rank of American soldiers, and know that if the same number of these American soldiers taken indiscriminately had been sorted according to their heights and marched up to the wall, each man of them would find the chalked line which he found opposite to him to be of exactly his own height. So far as I can judge from the run of the figures in the table, the error would never exceed a quarter of an inch, except at either extremity of the series.

The principle of the law of deviation is very simple. The important influences that acted upon each pellet were the same; namely, the position of the point whence it was dropped, and the force of gravity. So far as these are concerned, every pellet would have pursued an identical path. But in addition to these there were a host of petty disturbing influences, represented by the spikes among which the pellets tumbled in all sorts of ways. The theory of combination shows that the commonest case is that where a pellet falls equally often to the right of a spike as to the left of it, and therefore drops into the compartment vertically below the point where it entered the harrow. It also shows that the cases are very rare of runs of luck carrying the pellet much oftener to one side than the other. The law of deviation is purely numerical; it does not regard the fact whether the objects treated of are pellets in an apparatus like this, or shots at a target, or games of chance, or any other of the numerous groups of occurrences to which it is or may be applied.[1]

I have now done with my description of the law. I know it has been tedious, but it is an extremely difficult topic to handle on an occasion like this. I trust the application of it will prove of more interest.

(To be continued.)

ON THE STRUCTURE AND ORIGIN OF METEORITES[2]

THE study of meteorites is naturally divisible into several very distinct branches of inquiry. Thus in the first place we may regard them as shooting stars, and observe and discuss their radiant points and their relation to the solar system. This may be called the astronomical aspect of the question. Then, when solid masses fall to the ground, we may study their chemical composition as a whole, or that of the separate mineral constituents; and lastly, we may study their mechanical structure, and apply to this investigation the same methods which have yielded such important results in the case of terrestrial rocks. So much has been written on the astronomical, chemical, and mineralogical aspect of my subject by those far more competent than myself to deal with such questions, that I shall confine my remarks almost entirely to the mechanical structure of meteorites and meteoric irons, and more especially to my own observations, since they will, at all events, have the merit of greater originality and novelty. Time will, however, not permit me to enter into the detail even of this single department of my subject.

In treating this question it appeared to me very desirable to exhibit to you accurate reproductions of the natural objects, and I have therefore had prepared photographs of my original drawings, which we shall endeavour to show by means of the oxyhydrogen lime-light, and I shall modify my lecture to meet the requirements of the case, exhibiting and describing special examples, rather than attempt to give an account of meteorites in general. Moreover, since the time at my disposal is short, and their external characters may be studied to great advantage at the British Museum, I shall confine my remarks as much as possible to their minute internal structure, which can be seen only by examining properly prepared sections with more or less high magnifying powers.

By far the greater part of my observations were made about a dozen years ago. I prepared a number of sections of meteorites, meteoric irons, and other objects which might throw light on the subject, and my very best thanks are due to Prof. Maskelyne for having most kindly allowed me to thoroughly examine the very excellent series of thin sections, which had been prepared for him. During the last ten years my attention has been directed to very different subjects, and I have done little more than collect material for the further and more complete study of meteorites. When I have fully utilised this material I have no doubt that I shall be able to make the subject far more complete, and may find it necessary to modify some of my conclusions. I cannot but feel that very much more remains to be learned, and I should not have attempted to give an account of what I have so far done, if I had not been particularly asked to do so by Mr. Lockyer. At the same time I trust that I shall at all events succeed in showing that the microscopical method of study yields such well marked and important facts, that in some cases the examination of only a single specimen serves to decide between rival theories.

In examining with the naked eye an entire or broken meteorite we see that the original external outline is very irregular, and that it is covered by a crust, usually, but not invariably black, comparatively thin, and quite unlike the main mass inside. This crust is usually dull, but sometimes, as in the Stannern meteorite, bright and shining, like a coating of black varnish. On examining with a microscope a thin section of the meteorite, cut perpendicular to this crust, we see that it is a true black glass filled with small bubbles, and that the contrast between it and the main mass of the meteorite is as complete as possible, and the junction between them sharply defined, except when portions have been injected a short distance between the crystals. We thus have a most complete proof of the conclusion that the black crust was due to the true igneous fusion of the surface under conditions which had little or no influence at a greater depth than $\frac{1}{100}$th of an inch. In the case of meteorites of different chemical composition, the black crust has not retained a true glassy character, and is sometimes $\frac{1}{80}$th of an inch in thickness, consisting of two very distinct layers, the internal showing particles of iron which have been neither melted nor oxidised, and the external showing that they have been oxidised and the oxide melted up with the surrounding stony matter. Taking everything into consideration, the microscopical structure of the crust agrees perfectly well with the explanation usually adopted, but rejected by some authors, that it was formed by the fusion of the external surface, and was due to the very rapid heating which takes place when a body moving with planetary velocity rushes into the earth's atmosphere—a heating so rapid that the surface is melted before the heat has time to penetrate beyond a very short distance into the interior of the mass.

When we come to examine the structure of the original interior part of meteorites, as shown by fractured surfaces, we may often see with the naked eye that they are mottled in such a way as to have many of the characters of a brecciated rock, made up of fragments subsequently cemented together and consolidated. Mere rough fractures are, however, very misleading. A much more accurate opinion may be formed from the examination of a smooth flat surface. Facts thus observed led Reichenbach to conclude that meteorites had been formed by the

[1] Quetelet, apparently from habit rather than theory, always adopted the binomial law of error, basing his tables on a binomial of high power. It is absolutely necessary to the theory of the present paper, to get rid of binomial limitations and to consider the law of deviation or error, in its exponential form.

[2] Abstract of lecture delivered by H. C. Sorby, F.R.S., &c., at the Museum, South Kensington, on March 10.

collecting together of the fragments previously separated from one another in comets, and an examination of thin transparent sections with high magnifying powers and improved methods of illumination, proves still more conclusively their brecciated structure. The facts are, however, very complex, and some are not easily explained. Leaving this question for the present, I will endeavour to point out what appears to be the very earliest history of the material, as recorded by the internal structure.

It is now nearly twenty years since I first showed that the manner of formation of minerals and rocks may be learned from their microscopical structure. I showed that when crystals are formed by deposition from water or from a mass of melted rock, they often catch up portions of this water or melted stone, which can now be seen as cavities containing fluid or glass. We may thus distinguish between crystalline minerals formed by purely aqueous or by purely igneous processes; for example, between minerals in veins and minerals in volcanic lavas. In studying meteorites it appeared to me desirable, in the first place, to ascertain whether the crystalline minerals found in them were originally formed by deposition from water or from a melted stony material analogous to the slags of our furnace or the lava of volcanoes. One of the most common of the minerals in meteorites is olivine, and when met with in volcanic lavas this mineral usually contains only a few and small glass cavities in comparison with those seen in such minerals as augite. The crystals in meteorites are, moreover, only small, and thus the difficulty of the question is considerably increased. However, by careful examination with high magnifying power, I found well-marked glass-cavities, with perfectly fixed bubbles, the inclosed glass being sometimes of brown colour and having deposited crystals. On the contrary I have never been able to detect any trace of fluid-cavities, with moving bubbles, and therefore it is very probable, if not absolutely certain, that the crystalline minerals were chiefly formed by an igneous process, like those in lava, and analogous volcanic rocks. These researches require a magnifying power of 400 or 600 linear.

Passing from the structure of the individual crystals to that of the aggregate, we find that in some cases we have a structure in every respect analogous to that of erupted lavas, though even then there are very curious differences in detail. By methods like those adopted by Daubrée, there ought to be no more difficulty in artificially imitating the structure of such meteorites than in imitating that of our ordinary volcanic rocks. It is, however, doubtful whether meteorites of any considerable size uniformly possess this structure. The best examples I have seen are only fragments inclosed in the general mass of the Petersburg meteorite, which, like many others, has exactly the same kind of structure as that of consolidated volcanic tuff or ashes. This is well shown by the Bialystock meteorite, which is a mass of broken crystals and more complex fragments scattered promiscuously through a finer-grained consolidated dust-like ash.

Passing from this group of meteorites, which are more or less analogous to some of our terrestrial volcanic rocks, we must now consider the more common varieties, which are chiefly composed of olivine and other allied minerals. The Mezö Madaras meteorite is an excellent illustration, since the outline of the fragments is well seen, on account of the surrounding consolidated fine material being of dark colour. In it we see more or less irregular spherical and very irregular fragments scattered promiscuously in a dark highly consolidated fine-grained base. By far the larger part of these particles do not either by their outline or internal structure furnish any positive information respecting the manner in which they were formed, but careful examination of this and other analogous meteorites, has enabled me to find that the form and structure of many of the grains is totally unlike that of any I have ever seen in terrestrial rocks, and points to very special physical conditions. Thus some are almost spherical drops of *true glass* in the midst of which crystals have been formed, sometimes scattered promiscuously, and sometimes deposited on the external surface, radiating inwardly; they are, in fact, partially devitrified globules of glass, exactly similar to some artificial blow-pipe beads.

As is well known, glassy particles are sometimes given off from terrestrial volcanoes, but on entering the atmosphere they are immediately solidified and remain as mere fibres, like *Pele's hair*, or as more or less irregular laminæ, like pumice dust. The nearest approach to the globules in meteorites is met with in some artificial products. By directing a strong blast of hot air or steam into melted glassy furnace slag, it is blown into spray, and usually gives rise to pear-shaped globules, each having a long hair-like tail, which is formed because the surrounding air is too cold to retain the slag in a state of perfect fluidity. Very often the fibres are the chief product. I have never observed any such fibres in meteorites. If the slag be hot enough, some spheres are formed without tails, analogous to those characteristic of meteorites. The formation of such alone could not apparently occur unless the spray were blown into an atmosphere heated up to near the point of fusion, so that the glass might remain fluid until collected into globules. The retention of a true vitreous condition in such fused stony material would depend on both the chemical composition and the rate of cooling, and its permanent retention would in any case be impossible if the original glassy globule were afterwards kept for a long time at a temperature somewhat under that of fusion. The combination of all these conditions may very well be looked upon as unusual, and we may thus explain why grains containing true glass are comparatively very rare; but though rare they point out what was the origin of many others. In by far the greater number of cases the general basis has been completely divitrified, and the larger crystals are surrounded by a fine-grained stony mass. Other grains occur with a fan-shaped arrangement of crystalline needles, which an uncautious, non-microscopical observer might confound with simple concretions. They have, however, a structure entirely different from any concretions met with in terrestrial rocks, as for example that of oolitic grains. In them we often see a well-marked nucleus, on which radiating crystals have been deposited equally on all sides, and the external form is manifestly due to the growth of these crystals. On the contrary the grains in meteorites now under consideration have an external form *independent of the crystals*, which do not radiate from the centre, but from one or more places on the surface. They have, indeed, a structure absolutely identical with that of some artificial blowpipe beads which become crystalline on cooling. With a little care these can be made to crystallise from one point, and then the crystals shoot out from that point in a fan-shaped bundle, until the whole bead is altered. In this case we clearly see that the form of the bead was due to fusion, and existed prior to the formation of the crystals. The general structure of both these and the previously described spherical grains also shows that their rounded shape was not due to mechanical wearing. Moreover, melted globules with well-defined outline could not be formed in a mass of rock pressing against them on all sides, and I therefore argue that some at least of the constituent particles of meteorites were originally detached glassy globules, like drops of fiery rain.

Another remarkable character in the constituent particles of meteorites is that they are often mere fragments, although the entire body before being broken may originally have been only one-fortieth or one-fiftieth of an inch in diameter. It appears to me that thus to break such

minute particles when they were probably in a separate state, mechanical forces of great intensity would be required. By far the greater number of meteorites have a structure which indicates that this breaking up of the constituents was of very general occurrence.

Assuming then that the particles were originally detached like volcanic ashes, it is quite clear that they were subsequently collected together and consolidated. This more than anything else appears to me a very great difficulty in the way of our adopting Reichenbach's cometary theory. Volcanic ashes are massed together and consolidated into tuff, because they are collected on the ground by the gravitative force of the earth. It appears to me very difficult to understand how in the case of a comet there could be in any part a sufficiently strong gravitative force to collect the dispersed dust into hard stony masses like meteorites. If it were not for this apparent difficulty we might suppose that some of the facts here described were due to the heat of the sun, when comets approach so near to it that the conditions may be practically almost solar. Comets may and probably do contain many meteorites, but I think that their structure indicates that they were originally formed under conditions far more like those now existing at the surface of the sun than in comets.

The particles having been collected together, the compound mass has evidently often undergone considerable mechanical and crystalline changes. The fragments have sometimes been broken *in situ*, and "faulted;" and crystallisation has taken place, analogous to that met with in metamorphic rocks, which has more or less, and sometimes almost entirely, obliterated the original structure. The simplest explanation of this change is to suppose that after consolidation meteorites were variously heated to temperatures somewhat below their point of fusion. Those which have the structure of true lava may in some cases be portions which were actually remelted. We have also this striking fact, that meteoric masses of compound structure, themselves made up of fragments, have been again broken up into compound fragments, and these collected together and consolidated along with fresh material, to form the meteorites in their present condition. L'Aigle is a good example of this complex structure.

Another remarkable fact is the occurrence in some meteorites of many veins filled with material, in some respects so analogous to the black crust, that at one time I felt induced to believe that they were cracks, into which the crust had been injected. Akburfur is a good example of this, and seems to show that under whatever conditions the veins were found, they were injected not only with a black material, but also with iron and magnetic pyrites.

Taking, then, all the above facts into consideration, it appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; the particles could exist independently one of the other in an incandescent atmosphere, subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together. All these facts agree so admirably with what we know must now be taking place near the surface of the sun, that I cannot but think that, if we could only obtain specimens of the sun, we should find that their structure agreed very closely with that of meteorites. Considering also that the velocity with which the red flames have been seen to be thrown out from the sun is almost as great as tha necessary to carry a solid body far out into planetary space, we cannot help wondering whether, after all, meteorites may not be portions of the sun recently detached from it by the violent disturbances which do most certainly now occur, or were carried off from it at some earlier period, when these disturbances were more intense. At the same time, as pointed out by me many years ago, some of the facts I have described may indicate that meteorites are the residual cosmical matter, not collected into planets, formed when the conditions now met with only near the surface of the sun extended much further out from the centre of the solar system. The chief objection to any great extension of this hypothesis is that we may doubt whether the force of gravitation would be sufficient to explain some of the facts. In any case I think that one or other of these solar theories, which to some extent agree with the speculations of the late Mr. Brailey, would explain the remarkable and very special microscopical structure of meteorites far better than that which refers them to portions of a volcanic planet, subsequently broken up, as advocated by Meunier, unless indeed we may venture to conclude that the material might still retain its original structure, due to very different conditions, previous to its becoming part of a planet. At the same time so little is positively known respecting the original constitution of the solar system, that all these conclusions must to some extent be looked upon as only provisional.

I will now proceed to consider some facts connected with meteoric irons. The so-called Widmanstatt's figuring, seen when some of these irons are acted on by acids, is well known; but in my opinion the preparations are often very badly made. When properly prepared, the surface may be satisfactorily examined with a magnifying power of 200 linear, which is required to show the full detail. We may then see that the figuring is due to a very regular crystallisation, and to the separating out one from the other of different compounds of iron and nickel, and their phosphides. When meteoric iron showing this structure is artificially melted, the resulting product does not show the original structure, and it has therefore been contended that meteoric iron was never in a state of igneous fusion. In order to throw light on this question, I have paid very much attention to the microscopical structure of nearly all kinds of artificial irons and steels, by studying surfaces polished with very special care, so as to avoid any effect like burnishing, and then acting on them very carefully with extremely dilute nitric acid. In this manner most beautiful and instructive specimens may be obtained, showing a very great amount of detail, and requiring a magnifying power varying up to at least 200 linear. In illustration of my subject I will call attention to only a few leading types of structure. In the first case we have grey pig-iron, showing laminæ of graphite promiscuously arranged in all positions, on the surface of which is a thin layer of what is probably iron uncombined with carbon, whilst the intermediate spaces are filled up with what are probably two different compounds of iron and carbon.

White chilled refined iron has an entirely different structure and more uniform crystallisation, the structure is very remarkable and beautiful, mainly due to the varying crystallisation of an intensely hard compound of iron and carbon, and the two other softer compounds met with in grey pig.

Malleable bar iron has an entirely different structure, and shows fibres of black slag, and a more or less uniform crystallisation of iron with a varying small amount of carbon.

Cast steel differs again very much from any of the previous. It shows a fine-grained structure, due to small radiating crystals, and no plates of graphite.

The difference between any of the above and meteoric iron is extremely great.

In the case of Bessemer metal we have a crystalline structure approaching in some places more nearly to that of meteoric iron. We see a sort of Widmanstatt's figuring, but it is due to the separation of free iron from a compound containing a little carbon, and not to a variation in the amount of nickel.

The nearest approach to the structure of meteoric iron

is met with in the central portion of thick bars of Swedish iron, kept for some weeks at a temperature below their melting point, but high enough to give rise to recrystallisation. We then get a complete separation of free iron from a compound containing some carbon, and a crystalline structure which, as far as mere form is concerned, most closely corresponds with that of meteoric iron, as may be at once seen on comparing them.

These facts clearly indicate that the Widmanstatt's figuring is the result of such a complete separation of the constituents and perfect crystallisation as can occur only when the process takes place slowly and gradually. They appear to me to show that meteoric iron was kept for a long time at a heat just below the point of fusion, and that we should be by no means justified in concluding that it was not previously melted. Similar principles are applicable in the case of the iron masses found in Disco, and it by no means follows that they are meteoric because they show the Widmanstatt's figuring. Difference in the rate of cooling would serve very well to explain the difference in the structure of some meteoric iron, which do not differ in chemical composition; but, as far as the general structure is concerned, I think that we are quite at liberty to conclude that all may have been melted, if this will better explain other phenomena. On this supposition we may account for the separation of the iron from the stony meteorites, since under conditions which brought into play only a moderate gravitative force, the melted iron would subside through the melted stone, as happens in our furnaces; whilst at the same time, as shown in my paper read at the meeting of the British Association in 1864, where the separating force of gravitation was small, they might remain mixed together, as in the Pallas iron, and others of that type.

In conclusion I would say that though from want of adequate material for investigation I feel that what I have so far done is very incomplete, yet I think that the facts I have described will, at all events, serve to prove that the method of study employed cannot fail to yield most valuable results, and to throw much light on many problems of great interest and importance in several different branches of science.

MENDELEEF'S RESEARCHES ON MARIOTTE'S LAW[1]

FROM researches on the depression of the mercury results the possibility of introducing a precise correction relative to the volume of gas contained between the surface of the mercury and the horizontal plane which touches the summit of the meniscus. In all my researches I introduce each time a correction relative to this volume.

The volume of the reservoir which contains the mercury and the gas under various pressures undergoes two kinds of variations; first, those which are due to the difference between the pressures which act on the two sides of the vessel, and second, those which depend on differences in the volume of mercury. The compressibility of the reservoirs employed in the researches has been always determined by experiment, and their change of volume produced by the introduction of mercury can be determined by surrounding the vessel filled with mercury by another filled with the same material. When the height in the two vessels is the same, the capacity of the vessel is that which exists at the time of equality of pressure on the external and internal surfaces of the vessel. If we empty a part of the external vessel the capacity of the vessel changes in the same manner as when we fill or when we empty the vessel. Experiments of this kind have shown the possibility of determining the changes of capacity depending on the quantity of mercury. The relative corrections have in each case been introduced into the calculations.

All the practical side of the subject—the desiccation of the gas, the complete abstraction of the remains of the gas from the apparatus, the hermetical junction of the parts of the apparatus by means of mastic and mercury stop-valves, the means of maintaining the gases and the mercury at a constant temperature, the calibration of the tubes, and a number of other details have had to be elaborated more or less anew. All this will be found described in my work "On the Elasticity of Gases." I have published this work only in Russian, not having means sufficient to publish a translation of a work so voluminous, and desiring to conform to the custom existing among *savants* of all countries of describing their labours in their mother-tongue, in order to present to the scientific literature of the country where they live and work a gift in proportion to their powers.

My desire was to investigate the subject in its minutest details in order to eliminate every possibility of doubt as to the causes which determine the deviations observed from the Boyle-Mariotte Law. I know that that law is firmly established, and I believe it will remain so. Not less great is the certainty in the mind that rarefied gases approach the perfect state. That certainty I had also on commencing my experiments. It was necessary then to determine as completely as possible all the circumstances on which depend the facts contrary to the opinion generally held. This is why I have modified the apparatus, improved the methods, and employed in this work more than three years without interruption. Now so far as regards low pressures the work is finished, and I have obtained definitely certain proofs of the rigorous accuracy of my first observations.

The experiments which I have made with Kirpitchoff have proved that not only for air, but also for hydrogen, and even for carbonic acid, the deviations are positive when the gas is subjected to a very small pressure; it is found, moreover, that these deviations increase in proportion to the variation from the normal pressure. The same thing has been found in a new series of experiments undertaken by me with M. Hemilian. The experiments are described in tome ii. of my work on the "Elasticity of Gases," which I have just published. A brief extract on this subject is published in the *Ann. de Chimie et de Physique*, October, 1876. I shall quote only the results obtained by us from the experiments made in 1875 and in the beginning of 1876.

Into a new apparatus we have introduced several further improvements, of which the chief are:—(1) The baromanometer, the metre, and the reservoir, containing the gas and the mercury, have been placed in the same bath full of water; (2) We have succeeded in producing a complete vacuum in the barometric chamber; (3) The bath was maintained at an almost uniform temperature by means of an agitator, and the small differences in the temperatures of the various layers have been determined by a differential thermometer; (4) The junction between the air reservoir and the baromanometer has been made, not only without the aid of a tap, but also without the use of mastic.[1] Thus the gas was surrounded only by the glass and the mercury. We shall confine ourselves to a summary of the results of our experiments, made between 650 and 20 millimetres' pressure, with four gases — H, air, CO^2, and SO^2.

1. If, starting with a certain small pressure, we arrive at pressures smaller still, we find for all gases positive deviations, viz., $\frac{d(pv)}{dp} > 0$; the gases, then, are in this case less compressed than Mariotte's Law requires. Similar deviations were also observed for hydrogen by M. Regnault between 1 and 30 atmospheres, and M. Natterer for all gases between 100 and 3,000 atmospheres.

2. Under small pressures and for all gases, the value of the positive deviations, *i.e.*, the numerical quantity (or magnitude) $\frac{d(pv)}{dp}$, increases when the initial pressure diminishes. Thus, for example, for hydrogen at 400 millimetres—

$$\frac{d(pv)}{dp} = +0{\cdot}000002,$$

and at 120 millimetres—

$$\frac{d(pv)}{dp} = +0{\cdot}000010.$$

3. For gases like CO^2 and SO^2 we find near the atmospheric pressure, negative deviations; *e.g.*, for CO^2, $p_0 = 635$, $p_1 = 200$, $p_0 v_0 = 10{,}000$, $p_1 v_1 = 10{,}029$; but, under less pressures still, the deviations become positive even for CO^2 and SO^2. For example, for CO^2, $p_0 = 190$, $p_1 = 64$, $p_2 = 22$, $p_0 v_0 = 10{,}000$, $p_1 v_1 = 9{,}996$, $p_2 v_2 = 9{,}983$; for SO^2, $p_0 = 190$, $p_1 = 60$, $p_2 = 22$, $p_0 v^0 = 10{,}000$, $p_1 v_1 = 10{,}010$, $p_2 v_2 = 9{,}996$.

4. The existence of positive and negative deviations for the

[1] Continued from p. 457.

[1] To attain this end the gas-vessel and the branch of the baromanometer are soldered together by a capillary tube made of a single piece.

same gas, observed by means of the same apparatus, according to the amount of pressure, and the conformity in the various series of experiments, prove that the results obtained do not depend on any constant errors in the methods employed, but that they are really caused by the nature and the essential qualities of the gases investigated.

5. The variations from Mariotte's Law under very weak pressures being very small, it is necessary, in determining them, to make the reading of the pressures, the volumes, and the temperatures (absolute $t = 273°$) with a precision of two-thousandths of these total values; thus, *e.g.*, if $p_0 = 0{\cdot}200$ m., $p_1 = 0{\cdot}100$ m., and $v_0 = 2{,}500$ gr., $v_1 = 5{,}000$ gr. of mercury ($t = 20$), it will be necessary to determine the pressures with a precision of 0·01 mm., the volumes to 0·1 gr. of mercury, and the temperatures to 0·01 of a degree.

The results will be doubtful if the precision is less.[1] Thus it is found that *under a certain small pressure gases present positive deviations from Mariotte's Law;* even gases like sulphurous acid and carbonic acid, which under high pressures show considerable negative deviations. It is the same with air. M. Regnault commenced his researches with pressures which exceeded that of the atmosphere, and obtained negative deviations.

In 1874 I effected with all the care possible the determination of the deviations for air under pressures of from 650 to 2,000 milimetres, and towards the end of 1875 and in the beginning of 1876, in a special apparatus provided with compound manometers, I repeated the same experiments with M. Bogusski for pressures from 700 to 3,000 millimetres with air, hydrogen, and carbonic acid. These researches proved the rigorous accuracy of M. Regnault's conclusions. Air and carbonic acid were found to be subject under these pressures to negative deviations, greater for carbonic acid than for air; and hydrogen, for these same pressures, was found to present positive deviations. At present we are continuing the same kind of experiments for pressures of more than three metres.

Thus hydrogen, under all pressures, commencing with zero and ending with a pressure infinitely great, presents throughout positive deviations; at no pressure does it follow Boyle's Law rigorously, and it never presents negative deviations. Increased pressures always give a greater volume than what might be expected according to the variation of the pressures. Air under pressures less than 600 millimetres also presents positive deviations; but under pressures greater than that of the atmosphere its deviations become negative, and under pressures which exceed 100 atmospheres its compressibility again becomes positive. Consequently for this gas there are two pressures at which it follows Boyle's Law; the one is very nearly that of the atmosphere, the other lies between 30 and 100 atmospheres. These pressures, under which the changes of the sign of compressibility occur, will be different for carbonic acid; viz., under pressures less than that of the atmosphere the change of sign is found at nearly 200 millimetres, and for higher pressures it commences near that which corresponds to 70 metres of mercury, if we base our researches on this point on the observations of Dr. Andrews on the compressibility of carbonic acid gas for temperatures above 31°. For lower temperatures this point will probably correspond to the passage of carbonic acid into the liquid state. Consequently with a change of temperature the pressure at which the change of sign of compressibility occurs, changes also. For sulphurous acid the sign of compressibility under pressures lower than that of the atmosphere changes at about forty millimetres of pressure. But even this gas, so easily liquefiable, under low pressures, has always a positive compressibility. There is not then, and there cannot be, a gas which is rigorously subject to Mariotte's Law under small pressures.

The idea of an absolute gas belongs, then, to the number of fictions which find no confirmation in facts. We cannot, then, suppose that with the decrease of density or with the increase of the *vis viva* of gaseous molecules, gases approach a state in which they follow Boyle's Law. Then (the density diminishing, the velocity of the molecules increasing, that is to say, the pressure diminishing, the temperature increasing, and the molecular weight diminishing) they all tend towards another state characterised by the expression $\frac{d(pv)}{dp} > 0$; *i.e.*, they are assimilated to solid and liquid bodies, when the condensation reaches its limit. We must believe that there is a limit of condensation and a limit of rarefaction. If we take, in fact, a mass of non-volatile liquid, and if we submit it to pressures infinitely great and infinitely small, we shall see it change volume; but in the two cases, we shall have finite volumes, capable of measurement, and even differing little for one and the same body. It is the same with gases, if we admit that for pressures approaching zero, gases contract according to the same law as that which we can deduce from our compression experiments under pressures less than that of the atmosphere, or as hydrogen contracts. Under great pressures, or under pressures excessively small, every gas resembles a solid or liquid body, and possesses two limits of compressibility. The volumes which correspond to these limits are very different, but there is always reason for believing that they exist.

Without launching into hypotheses to explain these limit volumes (such, *e.g.*, as the supposition that molecules in themselves possess volume), I will confine myself to the question of the matter of celestial space. What is the luminous ether? One of two things—either an elastic independent matter, *sui generis*, or the gas of the atmospheres of celestial bodies, considerably rarefied. In the latter case it is necessary to admit the absence of limits in the atmospheres and a condensation of the ether greater and greater in proportion as we approach a celestial body (sun or planet). There are many arguments for and against both hypotheses. On the one hand, spectrum analysis leads us to conclude that the material of all heavenly bodies is identical; on the other hand, it proves the diversity of the composition of atmospheres. This is why we abstain from solving the question in its essence. But spectrum analysis does not speak less in favour of the former hypothesis, because it shows the diversity of composition of our terrestrial atmosphere from that of many of the other celestial bodies. In the researches on the resistance of celestial matter to the movement of the planets, there appears also to be a confirmation of the former of these two hypotheses, for neither planets nor comets show any diminution in the excentricity of their orbits, which would be an inevitable consequence of motion in a rarefied medium, as has been observed in the case of Encke's comet. Exact investigations on the movement of that comet, repeated in recent times by M. von Asten, the Pulkowa astronomer, show clearly the advances towards the sun at perihelion, although in the beginning M. von Asten had not noticed them. But that comet at perihelion was found only at one-third of the distance which separates the sun from the earth, *i.e.*, it was nearer to the sun than Mercury. It is possible that it passed near to the limits of the solar atmosphere. Faye's comet, as is known, does not present these same diversities, but its perihelic distance is about 1·68, that of Encke's comet being only about 0·33; it exceeds it then so much that their comparison would only serve to confirm the hypothesis of a solar atmosphere. If we admit a limit for the atmospheres, we must expect in gases, for small pressures, exactly that kind of variation from Boyle's Law which I observed in rarefied gases.

To prove that gases under very small pressures, as well as under very considerable pressures, vary from the Boyle-Marriotte Law is by no means the same as to deny the truth of that law; I feel that I ought to state this most explicitly. For a long time the law of gravitation could not be made to accord with the perturbations; latterly these perturbations have proved the best confirmation of the laws of gravitation. In the present case it may be the same. There are three laws for gases: that of Boyle and Marriotte, $pv =$ const.; that of Gay Lussac, $vt = v_0(1 + \alpha t)$; and that of Ampère and Gerland $\frac{av}{m} =$ const. (a being the molecular weight, and m the mass). Their *ensemble* is expressed for all gases in general by the equation—

$$apv = 845\,(273 + t)\,m,$$

where a is the atomic weight ($H = 1$), p the pressure in kilograms per square metre, v the volume in cubic metres, m the weight in kilograms, t the centigrade temperature. This is, however, only a first approximation. In the second member of the equation there must be additional terms which express a function of p and of a, very small for the ordinary mean values of p, and which become of a sensible magnitude only when p is very small or very great. To find this function is a question of the future, and demands the labours of a great number of investigators. My aim is to be able to furnish some experimental data which will permit of judging of the form of that function. This work requires many new processes, measuring apparatus of a high

[1] It is by these causes that the want of conformity in the experiments of Siljeström is sufficiently explained (*Pogg. Ann.*, April and May, 1874; see also the *Bull. de l'Acad. de Sc. de St. Pétersbourg*, t. xix, p. 466, and *Berichte der deutschen chem. Gesell.*, t. viii., p. 1,339; t. viii., pp. 576 and 749), and of M. Amagat (*Comptes Rendus*, April 17, 1876.)

degree of precision, many varied arrangements as well as experienced assistants. That idea has secured me the protection of His Imperial Highness the Grand Duke Constantine and the support of the Imperial Russian Technical Society.

I thus conclude this communication which I have the honour to send you. The researches on the co-efficients of dilatation of gases in their general features confirm the accuracy of the deductions drawn from the observations on compressibility. But that matter is in process of elaboration, and only a part of the researches has been completed. Consequently I abstain from expounding all the details of the subject. I shall only remark that the true coefficient of dilatation of air under constant pressure and with variable volume is found to be greater for pressures near to atmospheric pressure than the number generally accepted, notably from the researches which I have made with M. Kaïander, it is equal to 0·003683 if we take 100° C. as the temperature of boiling water under an atmospheric pressure of 760 mm. in latitude 45°.

If you do not find devoid of interest more ample details on this subject, as also on the determination of the weight of a litre of air, I shall have the greatest pleasure in explaining to the English public through your interesting journal the essential points of the researches made on this subject in my laboratory.

St. Petersburg, January 1 DE MENDELEEF

OUR ASTRONOMICAL COLUMN

BINARY STARS.—From one of the very careful and complete investigations on the orbits of the revolving double stars, by which Dr. Doberck is so greatly contributing to our knowledge of the motions of these interesting objects, we have an orbit of ξ Scorpii which is probably a very near approximation to the true one.

We have called the star by what appears to be its correct designation, ξ Scorpii, but few stars have been subjected to more varied nomenclature than the one in question. In Dr. Doberck's paper, published in No. 2,121 of the *Astronomische Nachrichten*, in all probability through one of the typographical errors which have of late so much disfigured this periodical, the star is styled ζ Libræ. It has been previously very commonly lettered ξ Libræ, and it is 51 Libræ of Flamsteed; Secchi, in *Astron. Nach.*, No. 1,614, calls it ρ' Libræ, though we are ignorant upon what precedent.

Dr. Doberck's elements are those of a very nearly circular orbit, and it may be remembered that some thirty years since Mädler gave elements for circular motion with a period of revolution of about 104 years. In *Astron. Nach.*, No. 1,683, Dr. Thiele gave the results of a very complete discussion of the measures up to 1856, in which he has assumed that Sir W. Herschel's angle on the night of his discovery of the duplicity of the star has been registered correctly, though a doubt has been entertained upon this point. He thus arrives at a highly excentrical elliptic orbit with a period of a little over forty-nine years. It should be remarked that from the near approach of the magnitudes of the components forming the double star ξ Scorpii (it is more correctly a triple star), an error of 180° in the measured angle of position is by no means an improbable one.

Dr. Doberck assumes that Sir W. Herschel's angle of 1782 requires this correction, and deduces an orbit in which the period of revolution is nearly twice that of Thiele, and which therefore approaches the period originally assigned by Mädler. His elements are as follow :—

Peri-astron passage 1859·62.

Node ... 12° 15′	Inclination	...	68° 42′
Node to peri-astron on orbit	...	...	89° 16′
Eccentricity	...	...	0·0768
Semi-axis major	...	...	1″·26
Revolution	...		95·90 years.

A full comparison with the measures up to the present time, appears in the *Astronomische Nachrichten*.

In the same number Dr. Doberck gives first elements of that exceedingly difficult object γ Coronæ Borealis, in which the period of revolution is 95½ years, and the peri-astron passage 1843·7. The distance calculated from this orbit is still under two-tenths of a second, but it will increase, until towards the end of the first decade of the next century the components, according to Dr. Doberck's calculation, will be separated by more than 0″·8.

THE ANNULAR ECLIPSE OF THE SUN, 1737, MARCH 1.—Prof. Grant, in his "History of Physical Astronomy," mentions this eclipse as the first annular one of which we have any detailed account. This phase passed over Edinburgh, where it was observed by Maclaurin, the well-known mathematician, by Short, the optician, Lord Aberdour, and others. The times were determined by Maclaurin by a pendulum clock of Graham's, and he was also furnished with a meridian instrument by the same maker, with the aid of which the clock was rated by Short for "a long time before and after the eclipse." The clock used by Lord Aberdour, who was located in Edinburgh Castle, was compared with Maclaurin's at noon on the day of the eclipse, and in addition signals were exchanged between the Castle and Maclaurin's station at the college. Both observers determined the duration of the annular phase to have been 5m. 48s.

The following elements of this eclipse have been deduced from a similar system of computation as regards the moon's place to that adopted for other eclipses to which reference has been made from time to time in this column, a system which gives results for the total solar eclipse of 1715 agreeing very closely with the observations of Flamsteed and Halley.

G.M.T. of conjunction in R.A., March 1, at 3h. 1m. 31s.

	°	′	″	
R.A.	342	35	32·0	
Moon's hourly motion in R.A. ...		28	50·9	
Sun's „ „ „ ...		2	20·3	
Moon's declination	6	44	0·1	S.
Sun's „	7	24	5·7	S.
Moon's hourly motion in decl. ...		8	49·5	N.
Sun's „ „ „ ...		0	57·2	N.
Moon's horizontal parallax		54	19·1	
Sun's „ „			9·0	
Moon's true semi-diameter		14	48·0	
Sun's „ „		16	7·7	

The equation of time was 12m. 40s. subtractive from mean time. The following were points upon the central line by the above elements :—

Long. 10° 10′ W.	Lat. 54° 53′ N.	Long. 4° 11′ W.	Lat. 56° 35′ N.
„ 7 26 W.	„ 55 42 N.	„ 0 4 W.	„ 57 36 N.

If reduction equations are founded upon a direct calculation for Edinburgh, there result for any place not far distant :—

$$\text{Cos}\, w = 27{\cdot}6369 - [1{\cdot}59611] \sin l + [1{\cdot}22018] \cos l, \cos (L - 51^\circ\ 1'{\cdot}4)$$
$$t = 2\text{h. } 13\text{m. } 35{\cdot}5\text{s.} \mp [2{\cdot}29350] \sin w + [3{\cdot}49325] \sin l - [3{\cdot}89289] \cos l, \cos (L + 147^\circ\ 35'{\cdot}5).$$

where l is the geocentric latitude of the place, L its longitude from Greenwich + if E, − if W, and t is expressed in Greenwich mean time; the upper sign is to be used for beginning of annular phase, and the lower for the ending. The quantities within square brackets are logarithms.

The calculated duration of annularity at Edinburgh is 5m. 46·6s., differing only 1·4s. from the observations of Maclaurin and Lord Aberdour, but the middle of this phase is given later by 1m. 51s. At other places mentioned in Maclaurin's memoir on this eclipse, published in the *Philosophical Transactions*, the duration of the annulus was as follows :—At Alnwick, 2m. 0s. ; at Crosby, near Ayr, 5m. 54s. ; at Montrose, 6m. 27s. ; and at St. Andrew's, 6m. 12s. At Aberdeen, which was very near the central line, the *annulus* was formed at 3h. 43m. 0s. local mean time, and continued 6m. 30s. On the east coast of Scotland, where the duration of *annulus* was longest, it did not exceed 6m. 35. The eclipse is not given annular at Morpeth, therein agreeing with the observation.

NOTES

THE Swiney Lectures on Geology (free to the public) will be delivered this year at the Royal School of Mines, Jermyn Street, on Monday and Thursday evenings at eight P.M., commencing on Monday, April 9.

AS we have already stated, the "Verein für die deutsche Nordpolarfahrt," of Bremen, has been converted into a Geographical Society (die geographische Gesellschaft in Bremen) with the object of promoting scientific exploring expeditions generally, and of publishing their results. Dr. Finsch, who was sent out by the Verein last year to the mouths of the Obi along with Dr. Brehm and Graf Waldburg-Zeil, is now busily engaged in working out the ethnographical and natural history collections made during the expedition and publishing their results. A paper of Dr. Finsch on the new birds discovered on this occasion has been received by the Zoological Society of London, and will be read at one of their next scientific meetings.

ON Friday, April 13, Mr. W. Spottiswoode will deliver a lecture at the Royal Institution on his great Induction Coil, described in the January number of the *Philosophical Magazine* and in the March number of the *Nineteenth Century*. The lecture will be illustrated with some new experiments on stratified discharges, which a coil of this enormous power has for the first time rendered practicable.

THE Leipsic publisher, Günther, has issued the first part of a new journal, *Kosmos*, specially devoted to the furtherance of the development doctrine, under the editorship of Caspari, Jäger, and Krause, with the assistance of Darwin, Haeckel, and other eminent workers in the Darwinian field. We shall give a detailed notice of the first part in an early number.

PROF. SYLVESTER seems to have become quite naturalised in the United States, if we may judge from the fact that he was one of the speakers at the celebration of Washington's birthday at the Johns Hopkins University, where, as our readers know, he is Professor of Mathematics. Prof. Sylvester spoke of his work and of his satisfaction with his new relations, as also on some points of general interest. He maintains that it is a mistake to divorce teaching and research, illustrating the advantage of their union in his own case by the fact that in the act of teaching, important fields for research have been suggested to him. Recently, for example, at the University in Baltimore, the persistence of a student in his desire to study the new algebra has led Prof. Sylvester into "a research of fascinating interest," from which he hopes for great results. He has reason "to think that the taste for mathematical study, even in its most abstract form, is much more widely diffused than is generally supposed" in the United States. Prof. Sylvester, the *Nation* tells us, spoke at considerable length and with deep feeling on the estrangement between the two great branches of the Anglo-Saxon race caused by the exclusive, ecclesiastical policy of the English Universities in former years. "Their work it is that a separation deeper and a chasm more difficult to fill up has been created between the two most free and powerful nations in the world—England and America—than any due to political causes, present or past." Why is it, he inquired, that the flower of American youth do not resort for their mental impulse and higher education to Oxford and Cambridge, instead of to Berlin, Leipzig, Jena, or Heidelberg? "It is because there they are welcomed, to whatever religious communion they are attached or unattached, without question and without distinction. It is because there they can rest on the bosom of a common mother, who shows kindness to all and favour to none. . . I have been struck, almost from the first hour of my landing on these shores, by the manifestations I have everywhere witnessed of the close intellectual sympathy, which exists between America and Germany. It is German books that are read, it is German authors who are quoted, German opinion on all matters of science and learning that is appealed to; and as regards community of work and intellectual ties, I do not think it at all extravagant to assert that Germany and America belong to one hemisphere, and we in England to another." If our universities are to blame for this state of things, they have much to answer for; and it is therefore some relief to know that the New York *Nation* accounts for it by the desire of Americans to acquire another language in the country where it is spoken, and to come in contact with a different order of mind, however little superior.

DR. KLEIN, extraordinary professor of mineralogy in the University of Heidelberg, has accepted an ordinary professorship in the University of Göttingen.

THE Clothworkers' and Merchant Taylors' Companies have each contributed one hundred guineas to the fund being raised by the Chemical Society for the promotion of chemical research.

THE trustees of the Johnson Memorial Prize for the encouragement of the study of astronomy and meteorology propose the following subject for an essay, "The History of the Successive Stages of our Knowledge of Nebulæ, Nebulous Stars, and Star clusters, from the Time of Sir William Herschel." The prize is open to all members of the University of Oxford, and consists of a gold medal of the value of ten guineas, together with so much of the dividends for four years on 338*l*. Reduced Annuities as shall remain after the cost of the medal and other expenses have been defrayed. Candidates are to send their essays to the registrar of the University, under a sealed cover marked "Johnson Memorial Prize Essay," on or before March 31, 1879, each candidate concealing his name, distinguishing his essay by a motto, and sending at the same time his name sealed up under cover with the same motto written upon it.

ONE or more minor scholarships in natural science will be offered by Downing College, Cambridge, during the present year. The scholarships range in value from 40*l*. to 70*l*. per annum, and are tenable for two years, or until the holder is elected to a foundation scholarship. The examination will be held in Downing College on June 5 and the three following days. The subjects of examination will be (1) Chemistry, theoretical and practical; (2) Physics; (3) Comparative Anatomy; and (4) Physiology. All persons are eligible to these scholarships who have not commenced to reside in the University.

ON March 7, at Gjesvär, a Norwegian fishing station, near the North Cape, in 71° 12′ N. lat., the most northerly telegraph station on the earth was opened.

THE *Conversazione* of the Quekett Club takes place on the 13th inst., at University College, Gower Street.

THREE electric eels from the River Amazon have this week been added to the Westminster Aquarium. As they require to be kept at a temperature of between 70° and 80° F. it needed some ingenuity to bring them from Liverpool, where they were landed, to London. By placing the vessel containing them on foot-warmers and telegraphing on for changes of foot-warmers at different stations, the water on arriving at Westminster was found to be at 75°. The eels are lodged in a tank kept warm by a steam pipe passing under the shingle, and are at present by the alligators. These, by the by, are waking up wonderfully in activity, and the attendants have now to keep a sharp lookout when cleaning the tank.

WE are enabled to state that the increasing number of demands for space in the Paris International Exhibition has led M. Krantz reluctantly to give up the idea of authorising the con-

struction of the large Giffard Captive Balloon within the precincts of the Exhibition. The construction will take place at all events either on public ground lent by the Government or on some private vacant space at a small distance from the Champ de Mars. The preliminary technical arrangements have been made by M. Giffard. The length of the rope will be about 600 metres. It will be conical; the largest end close to the car will be 8 centimetres diameter, the smallest end only 6. The ascending force, when loaded with ballast, guide ropes, grapnels, and 50 passengers, will be 5 tons. The weight of the cable will be $2\frac{1}{2}$ tons when fully expended. The ascending force of the hydrogen filling the envelope will be 23 tons. The diameter of the balloon will be 34 metres, the height 50 metres from the lower part of the car to the upper part of the valve, and the engine will be of 200-horse power.

THE supplementary number, 51, of Petermann's *Mittheilungen* contains the second half of the late E. De Pruyssenaere's travels in the region of the White and Blue Nile. This part contains the special scientific results of the accomplished traveller—meteorological observations, barometrical altitude observations, river measurements, astronomical observations, triangulation of a part of the Jezîra, besides the southern half of the map, constructed by the editor, Herr Zoeppritz, and a plate of some of the implements, weapons, utensils, ornaments, &c., used by the inhabitants of the region traversed.

AT the meeting of the delegates of the French learned societies to be held at the Sorbonne, as we noticed last week, M. Alluard, the director of the Puy-de-Dôme Observatory, will present a most interesting paper. A self-registering barometer has been kept in constant operation on the top of the Puy-de-Dôme, and another similar instrument was observed at Clermont-Ferrand during the same length of time. The difference of pressure has undergone most remarkable variations, which cannot be accounted for by the Laplace law for determining the altitudes by comparing barometers. The corrections of temperature will be shown to be quite insufficient.

THE Scientific Congress of France, a quite distinct organisation, established by the late M. de Caumont, will hold its forty-third session at Versailles, from May 17 to 27. A number of attractive excursions have been arranged with the help of the municipal authorities, and there will be a floral exhibition.

AT the meeting of the St. Petersburg Society for the Protection of Trade, March 21, the maps prepared last summer by M Orloff during his journey to the Baydaraksky Gulf, were exhibited. The survey and levelling were made from the Irtish, up the Shchuchya River, and along the Baydaraka River to the Baydaraksky Gulf. Both rivers are navigable during the three months—June, July, and August.

A TOMSK telegram received by M. Siderof on March 18, from M. Schwanenberg's expedition, announces the find, on the banks of the Obi, near to the Mariinsky gold-washings, of a well-preserved mammoth with flesh and skin. The definitive excavation of the carcass was stopped until instructions should arrive from St. Petersburg.

DR. J. F. BRANSFORD, surgeon in the United States Navy, has been investigating the antiquities on the island of Omotepe, in Lake Nicaragua, collecting large numbers of vases of various kinds, burial urns, ornaments, and other objects for the National Museum at Washington. Among the more important points substantiated by him was the occurrence on the island of at least three successive and distinct bases of prehistoric civilisation, all of them anterior to the present epoch, these being bounded and defined by successive overflows of lava from the volcano. Very great intervals of time elapsed between the eruptions, as is shown by the accumulations of soil that took place on the fresh surface of the lava from the decomposition of vegetable deposits. No estimate can be made of these eras, but they are believed to carry the period of the earliest overflows back to a very remote antiquity. The objects of these successive layers are very definite and easily recognisable by the practised eye, and highly important deductions in regard to the early civilisation of that region are expected from a critical investigation of the subject. Dr. Bransford has prepared an elaborate report on this subject for presentation to the Navy Department, but, before publishing it, he has obtained permission to revisit the country, and settle some still doubtful points.

A MALVERN correspondent writes that he and many other residents in that part of the country are desirous of having some legislative protection for the *eggs* of such birds as are mentioned in the Wild Birds' Preservation Act. He wishes to know if there is any society for looking to the interests of wild birds; if so he and others will be glad to subscribe. The Woolhope Field Club used to give rewards for the best collection of birds' eggs, but the rule was altered when the mischief of this course as regards ornithology became evident.

THOSE of our readers who were at the Glasgow meeting of the British Association last autumn will, no doubt, remember the interesting collection which was on view in the City Industrial Museum. The Report of the Museum for 1876 has just been issued, and we are pleased to see that under the management of its Curator, Mr. Paton, it is rapidly increasing in size and importance, and we have no doubt that ere long it will become, what so important a city as Glasgow ought to possess, a really valuable industrial collection arranged on a thoroughly scientific plan.

AT a recent meeting of the French Academy M. de Romilly called attention to some remarkable effects obtained by suspension of water sucked up into a bell jar closed below by a tissue with wide meshes; in one arrangement, the net being metallic the suspended water could even be boiled by heat applied below. M. Plateau has just pointed out that he described this phenomenon of suspension in 1867, in treating of the construction of aquatic arachnida.

A propos of the question (which has been disputed) whether toads eat bees, M. Brunet states, in *La Nature*, that going one day into his garden, just before a storm, he found the bees crowding into their hives. About fifty centimetres from the best hive there was a middle-sized toad, which every now and again rose on his fore-legs and made a dart with surprising quickness towards blades of grass. He was found to be devouring bees, which rested on the grass-blades, awaiting their chance to enter the hive. M. Brunet watched till twelve victims had been devoured; he expected the toad's voracity would soon be punished with a sting, but in vain. Objecting to further destruction, he seized the toad by one of his legs and carried him to a bed of cabbage thirty metres off, where he might do real service among the caterpillars, &c. Three days after this, on going out to the hives, he found the same toad (which was easily distinguishable) at its old work. M. Brunet let him swallow only three or four bees, then carried him fifty metres in another direction. Two days later the "wretch" was again found at his post, greedily devouring.

OUR correspondent, "J. H.," in describing the path of the meteor of March 17, as seen by him at Rossall, near Fleetwood, wrote ϵ Hydræ for α Hydræ. The date of Mr. Ainslie Hollis's letter should have been March 19.

MR. ELLIS asks us to state that in his article on Musical Notation last week, p. 476, col. ii., lines four and five, the readings should be A_1 *fl*, A_2 *ssh*, G_2.

THE additions to the Zoological Society's Gardens during the past week include a Large-eared Brocket (*Cervus auritus*) from South America, presented by Mr. Charles Cooper; two Common Otters (*Lutra vulgaris*), European, presented by Mr. Augustus B. Foster; a Vulpine Phalanger (*Phalangista vulpinu*) from Australia, presented by Mr. Thos. Welsh; two Rufous Tinamous (*Rhynchotus rufescens*) from South America, presented by Mr. F. Searle Parker; eighteen Roach (*Leuciscus rutilus*), six Perch (*Perca fluviatilis*), six Tench (*Tinca vulgaris*), a Bream (*Abromis brama*), a Prussian Carp (*Carassius vulgaris*) from British fresh waters, presented by Mr. J. Smith; three Fire-tailed Finches (*Erythrura prasina*) from Sumatra, purchased; a Feline Dourocouli (*Nyctipithecus felinus*), a Kinkajou (*Cercoleptes caudivolvulus*), three Blue-shouldered Tanagers (*Tanagra cyanoptera*), an Adorned Terrapin (*Clemmys ornata*) from South America, deposited; a Great Kangaroo (*Macropus giganteus*), a Yellow-footed Rock Kangaroo (*Petrogale xanthropus*), a Collared Fruit Bat (*Cynonycterus collaris*), a Black Swan (*Cygnus atratus*) born in the Gardens.

SOCIETIES AND ACADEMIES

LONDON

Royal Society.—March 1.—"Note on the Electrolytic Conduction of some Organic Bodies," by J. H. Gladstone, Ph.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and Alfred Tribe, F.C.S., Lecturer on Chemistry in Dulwich College.

Our results, preliminary as we considered them to be, show that the iodides of ethyl, isobutyl, and amyl, the bromides of ethyl and propylene, the acetate of ethyl, and chloroform are practically non-conductors to a battery-power of 100 cells Grove, and that alcohol is to some extent traversed by the current. They show also that when these liquid non-conductors are mixed with the feeble conductor, alcohol, the conductivity of the mixture is greater than that of alcohol alone, which offers at least a partial clue to the readiness with which such mixtures are decomposed by the copper-zinc couple.

The very considerable development of heat in these liquids, which conduct the electric current with great difficulty, is a circumstance worthy of notice. In these cases it is evident that it does not result from any chemical change, because the decomposition, if anything at all, is utterly insignificant in amount.

"On the Protrusion of Protoplasmic Filaments from the Glandular Hairs of the Common Teasel," by Francis Darwin. Communicated by Charles Darwin, F.R.S.

The following is a summary of the results arrived at by Mr. Darwin:—Certain observations have been made on the protrusion of protoplasmic filaments, from leaf-glands on the teasel; and the only theory which seems at all capable of connecting these facts is the following. That the glands on the teasel were aboriginally (*i.e.*, in the ancestors of the Dipsacaceæ) mere resin excreting organs. That the protoplasm which comes forth was originally a necessary concomitant of the secreted matters, but that from coming in contact with nitrogenous fluids it became gradually adapted to retain its vitality and to take on itself an absorptive function. And that this power—originally developed in relation to the ammonia in rain and dew—was further developed in relation to the decaying fluid accumulating within the connate leaves of the plant.

March 8.—"On the Structure and Development of Vascular Dentine," by Charles S. Tomes, M.A. Communicated by John Tomes, F.R.S.

March 15.—"On the Density of Solid Mercury," by Prof. J. W. Mallet, University of Virginia. Communicated by Prof. Stokes, Sec. R.S.

The author gets 14·1932 *as the number representing the density of solid mercury at its fusing point as referred to water at 4° C. taken as unity.* This result, which differs considerably from previous figures, he thinks, may be fairly accepted with confidence.

"The Automatic Action of the Sphincter Ani," by W. R. Gowers, M.D., Assistant Physician to University College Hospital. Communicated by J. S. Burdon Sanderson, M.D., F.R.S.

"Description of the Process of Verifying Thermometers at the Kew Observatory," by Francis Galton, F.R.S.

Linnean Society, March 15.—Prof. Allman, president, in the chair.—The Rev. A. Gardner Smith and Mr. A. Y. Stewart were elected Fellows.—The Secretary read a paper on the poisoned spears and arrows of the Samoa Islanders, by the Rev. Thos. Powell. The information thereon had been derived from the son of a native chief. According to his account, the weapons are pointed with human thigh and parietal bones, these being ground to a fine tapering point. A milky juice, the product of several kinds of trees—among others *Callophyllum inophyllum*—is used for dipping the arrow and the spear-heads into, and there is added a substance obtained from wasps' nests, besides some of the fluid of putrid Sea-cucumbers (*Holothuria*). A kind of kiln is then prepared, where the weapons are smoked, after which they are inserted into the dried flower-stalk of a species of *Tacca*, to prevent bad effects from humidity; lastly, they are bundled together and laid by ready for use. The effects of the poison on the human system—viz., convulsions and tetanus, and the reputed means of cure the author duly mentions. Mr. G. Busk, however, questions the active quality of the said poison; at least some experiments of his incline him to think that a local irritation may be set up rather than an immediate deadly influence of a virulent vegetable poison, such as is the "Woorali" of South America. On the other hand, Messrs. Nichols and Pratt corroborate Mr. Powell's statements.—Dr. A. Gunther gave a notice of two large extinct lizards formerly inhabiting the Mascarene Islands. The remains of the bones had been partly obtained by Mr. Edward Newton, already well known for his researches on the extinct Mascarene fauna, and partly by Mr. H. H. Slater, Naturalist to the Transit of Venus Expedition. Comparisons have led Dr. Gunther to regard one relatively large animal as most nearly allied to the families of Zonuridæ and Scincidæ. But it differs both from the Glass Snakes and Skinks, hence a new genus has been assigned it and the name *Didosaurus mauritianus* given. The remains of another form from Rodriguez shows it to be allied and indeed identical with the Geckos, close to *G. verus* but specifically distinct, and accordingly named *G. newtonii*.—The second part of contributions to the ornithology of New Guinea, by Mr. R. Bowdler Sharpe, dealt with a collection made by the late Dr. James. This young enthusiastic naturalist was murdered by the natives during an expedition to one of the islands in Hall's Sound, whither he had gone to collect Birds of Paradise. Of fifty-three species obtained only three are new to science, and from this it is inferred that the south-eastern province visited has by no means so rich an avifauna as the northern parts of New Guinea are known to possess. The new species are *Melidora collaris, Phonygama jamesii*, and *Tanysiptera microrhyncha*. But a still more interesting night-flying black hawk, *Machaeramphus alcinus*, has turned up in this locality, whose habitat previously was supposed only to be Malacca and Tenasserim. Only four specimens of this rare bird are known to exist.—Samples of supposititious "manna" from Persia, and a bark (Leptospermum ?) from New Zealand, with tonic qualities, were exhibited and remarked on by Mr. Stewart, of the Apothecaries Hall.

Zoological Society, March 20.—Dr. E. Hamilton, vice-president, in the chair.—Mr. Sclater called the attention of the meeting to an article in the *Oriental Sporting Magazine* for May, 1876, by which it appeared that a two-horned rhinoceros had been killed in February, 1876, at a place some twenty miles south of Comillah, in Tipperah. Mr. Sclater stated that this was the third recorded occurrence of a two-horned rhinoceros north of the Bay of Bengal.—Mr. Sclater also called attention to the fact that Mr. W. Jamrach had just imported a young living specimen of the rhinoceros of the Bengal Sunderbunds, which was either *Rh. sondaicus* or a very closely allied form.—Mr. Sclater exhibited a small living Amphisbænian (*Blanus cinereus*), which had been accidentally brought to England in the roots of a hot-house plant from Port St. Mary, Spain.—Messrs. Charles G. Danford and Edward R. Alston read a paper on the mammals of Asia Minor, based principally on collections made by the former in that country. The list included one species of Bat, two of Insectivores, twenty of Carnivores, seven of Ungulates, and fourteen of Rodents. *Spermo philusxanthoprymnus*, Benn., was redescribed, and the name *Mus mystacinus* was proposed for a new species of field-mouse.—Mr. A. G. Butler read a paper on the Myriopoda obtained by the Rev. G. Brown in Duke of York Island. The species sent home were two in number, both of them allied to but distinct from previously described species. Mr. Butler proposed to designate them as *Heterostoma browni* and *Spirobolus cinctipes*.—A com-

munication was read from the Rev. O. P. Cambridge, in which he gave the descriptions of some spiders collected by the Rev. G. Brown in Duke of York Island, New Britain, and New Ireland. Two of these appeared to be undescribed, and were named *Argiope browni* and *Sarotes vulpinus*.—Prof. A. H. Garrod read a paper containing notes on the anatomy of the Musk Deer (*Moschus moschiferus*).—A communication was read from Mr. Edward Bartlett, containing remarks on the affinity of *Mesites* and the position which it should occupy in a natural classification. From an examination of structure of the feathers, Mr. Bartlett had come to the conclusion that *Mesites* was an aberrant form of the Ardeine group.—Dr. Günther, F.R.S., read a paper containing an account of the fishes collected by Capt. Feilden during the last Arctic Expedition. Amongst these were several of great interest, especially a new species of Charr, for which the name *Salmo arcturus* was proposed. This Charr was discovered in freshwater lakes of Grinnell-land, and was stated to be the most northern fresh-water fish known to exist.—Mr. Edward Newton, C.M.G., exhibited and read a paper on a collection of birds made in the island Anjuan or Johanna, one of the Comorro group, by Mr. Bewsher, of Mauritius, whereby the number of species now known to have occurred in that island was raised to thirty-five, of which fourteen were first observed there by that gentleman. Five of these, namely, *Zosterops anjuanensis*, *Tchitrea vulpina*, *Ellisia longicaudata*, *Turdus bewsheri*, and *Turtur comorensis*, were described as new.

Meteorological Society, March 21.—Mr. H. S. Eaton, M.A., president, in the chair.—Capt. Fellowes, R.E., George Jinman, Angus Mackintosh, M.D., Robert W. T. Morris, Rev. Edward Vincent Pigott, David S. Skinner, L.R.C.P., and Henry St. John Wood were elected Fellows of the Society.—The following papers were read:—Results of meteorological observations made at Patras, Greece, during 1874 and 1875, by the Rev. Herbert A. Boys. This is in continuation of a former paper read before the Society in 1875. The period embraced in the two papers—January, 1873, to June, 1875—covers a whole winter compressed into about thirty days, a very long and showery spring, an excessively hot summer, a dry winter of extreme cold, a summer of most prolonged drought, a remarkably wet and snowy winter, a very late beginning of hot weather, and the coldest day and night, and the lowest barometer reading for many years.—Contributions to the meteorology of the Pacific—Fiji, by Robert H. Scott, F.R.S. This paper contains a discussion of all published information as to the climate of Fiji which the author has been able to discover.—Local diurnal range, by S. H. Miller, F.R.A.S.—This was followed by another paper on the same subject, by William Marriott, F.M.S., which discussed the questions of whether the tables of corrections for diurnal range, at present used by a large number of observers, are trustworthy, and whether they are applicable to different places in the United Kingdom. The conclusions arrived at were that the present corrections could not be considered as accurate, that no strictly comparable records exist for instituting a satisfactory inquiry, and that it is very undesirable to apply any corrections whatever to the observations to deduce means from them.—Mr. Negretti exhibited several new instruments.

PARIS

Academy of Sciences, March 26.—M. Peligot in the chair.—The following papers were read:—Remarks on the presence of benzine in coal gas, by M. Berthelot. The illuminating portion of the Parisian gas consists mostly of vapour of benzine, forming about 3 per cent. of the whole volume. Fuming nitric acid was employed in the analysis, producing nitrobenzine.—On a recent communication of Mr. Weddell regarding the advantage to be realised in replacing quinine by cinchonidine, by M. Pasteur. Mr. Weddell having stated that cinchonidine was discovered by M. Pasteur, the latter says this is attributing too much to him, and defines his researches on the subject in 1853.—On the digestion of albumen, by M. van Tieghem. The relation of the albumen to the embryo in seeds was studied by two methods—observing isolated albumen subjected to germination and observing the dissolution of albumen during germination, of the entire seed. There are two modes of digestion; the oleaginous and aleuric albumen has an activity of its own, it digests itself, and the embryo only absorbs the products of this interior digestion; it is a "nurse" to it. The amylaceous and cellulosic albumens, on the contrary, are passive; they are digested by the embryo, each in its fashion, and the products of this external digestion are then absorbed by it; they are to it only a nutriment.—On preventive and early trepanation in vitreous fractures complicated by splinters, by M. Sedillot.—Observations of the satellites of Saturn, at the Observatory of Toulouse in 1876, with the large Foucault telescope, by M. Tisserand. These relate to the first five satellites only. From observations of three of them the apparent diameter of Saturn's ring is inferred to be 40″·51.—On a theorem relative to the expansion of vapours without external work, by M. Hirn.—On the theory of plane elastic plates, by M. Levy.—The president of the Vine-growing Society of the Pyrenées Orientales sent a document affirming that it is the American plants that have brought phylloxera into France; all plantation of them is the signal of a fresh invasion.—On the theory of frigorific machines, by M. Terquem.—On the reflection of polarised light, by M. Croullebois. He studies one of the fringes discovered by Airy, and named by M. Billet the *courbe de semelle*; showing what may be inferred from it, as to the physical constitution of a mirror (*i.e.*, its positive, neutral, or negative nature); the value of the angle of maximum polarisation (first constant), and the azimuth of renewed polarisation (second constant).—On the transformation of crystallisable sugar into inactive glucose in raw cane sugars, by M. Gayon. Heat and moisture favour the transformation; there is a real fermentation, with carbonic acid given off. By the mere decrease of crystallisable and increase of uncrystallisable sugar, the yield in refining was diminished by 25 per cent. in one sugar, and 33 per cent. in another.—On the composition of gun-cotton, by MM. Champion and Pellet. The specimen analysed contained (ashes deducted 1·0 gr. per cent.) free cellulose, 1·00; dinitrocellulose, 6·00; principal nitrated product (by difference), 93·00. Supposing this product pentanitrocellulose, and calculating the constituents on this hypothesis, we have, carbon, 26·54; hydrogen, 2·79; nitrogen, 12·51; oxygen, 58·16; which analysis confirms.—Studies on the series of the quinolines; transformation of leucoline into aniline, by Mr. James Dewar.—On nitrototuquinone and chloranilic acid, by M. Etard.—On the sewage waters of Paris, by M. Lauth. The facts cited prove that the sulphydric putrefaction of such water may be avoided by addition of lime, or (a much more important result) by simple aëration. Putrefaction only occurs when the sewage water is kept out of contact with air. As such conditions probably occur at the bottom of the Seine, the facts related may be utilised for its sanitation.—On the fecundation of the egg in the sea-urchin, by M. Perez. He questions M. Fol's statement that the spermatozoids penetrate into the interior.—Hailstorm at the Cape of Antibes on March 21, by M. Ferrière. The storm came from the depths of the marine horizon; its movement was from west to east, and the hailstones, judging from the orientation of the deposits, must have had a gyratory motion. These facts seem to bear on M. Faye's theory.—Chronic anæmia from stubborn nervous and digestive disorders continuing for five years; transfusion of blood and cure, by M. Oré. Only forty grammes of blood were used. Puncture was made without denudation of the vein. The transfused blood acts by stimulating the organs rendered atonic, and by causing a *proliferation* of new globules.—On the antiseptic properties of bichromate of potash, by M. Laujorrois. The addition of $\frac{1}{100}$ to ordinary water will render this conservative of all organic products without decomposition, even in free air.

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ERRATUM.—P. 466, col. 2, line 26, for "Garrell" read "Yarrell."